THE TRANSACTIONS

OF THE

MEDICO-CHIRURGICAL SOCIETY OF EDINBURGH.

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OF THE

MEDICO-CHIRURGICAL SOCIETY OF EDINBURGH.

VOL. VII.—NEW SERIES.

SESSION 1887-88.

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1888.

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PREFACE.

The present Volume is the *Seventh* of the *New Series*, and contains a record of the work done during the past Session.

That work, as hitherto, embraces the communication of Original Papers; the exhibition of Patients, illustrating rare and interesting forms of disease; and the exhibition of Pathological and other specimens, so essential to the proper understanding of the morbid changes which take place in the human body.

During the past Session two Extra Meetings were held for Discussions on Special Subjects,—one on the Etiology of Tumours, introduced by Dr G. Sims Woodhead, Superintendent of the Royal College of Physicians' Laboratory; and the other on Animal Tuberculosis in relation to Consumption in Man, introduced by Principal Walley of the Edinburgh Veterinary College. It is hoped that such Meetings will materially increase the usefulness of the Society.

It is believed that the publication of the Transactions in this permanent form will prove a valuable contribution to medical literature, will encourage the Members to take a more active part in the work of the Society, and will tend in no small degree to increase the influence and usefulness of the Medico-Chirurgical Society of Edinburgh.

William Craig,
Editor.

September 1888.

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	James Rutherford Morison, M.D., F.R.C.S. Ed., 14 Saville Row, Newcastle-on-Tyne,	1882
	Roderick Maclaren, M.D., 23 Portland Square, Carlisle,	1882
	W. Wotherspoon Ireland, M.D., Prestonpans,	1883
135	Francis Mitchell Caird, M.B., F.R.C.S. Ed., 21 Rutland Street,	1883
	F. W. Dyce Fraser, M.D., F.R.C.P. Ed., South Lodge, Ascot, Berks,	1883
	Robert Henry Blaikie, M.D., F.R.C.S. Ed., 9 Palmerston Road,	1883
	R. M'Kenzie Johnston, M.D., F.R.C.S. Ed., 5 Rutland Square,	1883
	Charles Walker Cathcart, M.B., F.R.C.S. Ed., 8 Randolph Crescent, *Secretary*,	1883
140	Alexander Bruce, M.D., F.R.C.P. Ed., 13 Alva Street,	1883
	Andrew Semple, M.D., F.R.C.S. Ed., Dep. Surgeon-General, 10 Forres Street,	1883
	William Hy. Shirreff, M.B., C.M., Melbourne, Australia,	1883
	John Lyon Wilson, L.R.C.P. Ed., 4 Buccleuch Place,	1883
	Donald MacRaild, F.R.C.S. Ed., Greenock,	1883
145	Henry Newcombe, M.D., F.R.C.S. Ed., 5 Dalrymple Crescent,	1883

		Date of Admission.
	* Francis Troup, M.D., M.R.C.P. Ed., 1 Minto Street,	1883
	Russell Eliott Wood, M.B., F.R.C.S. Ed., 9 Darnaway Street,	1883
	John Macdonald Brown, M.B., F.R.C.S. Ed., 12 South Mansionhouse Road,	1883
	James William Beeman Hodsdon, M.D., F.R.C.S. Ed., 30 Walker Street,	1883
150	John Haddon, M.D., C.M., Melrose,	1883
	Germon Sims Woodhead, M.D., F.R.C.P. Ed., 6 Marchhall Crescent,	1883
	Thomas Francis Spittal Caverhill, M.B., F.R.C.P. Ed., 8A Abercromby Place,	1883
	Robert Alexander Lundie, M.B., B.Sc., F.R.C.S. Ed., 35 Warrender Park Road,	1883
	Professor Arthur W. Hare, M.B., F.R.C.S. Ed., M.R.C.S. Eng., Owens College, Manchester,	1883
155	Edwin Baily, M.B., C.M., Oban,	1883
	Alexander Black, M.B., F.R.C.P. Ed., 8 St Vincent Street,	1883
	Harry Melville Dunlop, M.D., F.R.C.P. Ed., 20 Abercromby Place,	1883
	George Andreas Berry, M.B., F.R.C.S. Ed., 23 Rutland Street,	1883
	Hamilton Wylie, M.B., C.M., 1 George Place,	1883
160	Arthur Douglas Webster, M.D., M.R.C.P. Ed., 20 Newington Road,	1883
	Robert William Philip, M.D., F.R.C.P. Ed., 4 Melville Crescent,	1883
	Joseph Carne Ross, M.D., F.R.C.P. Ed., Penzance,	1884
	William Russell, M.D., F.R.C.P. Ed., 46 Albany Street,	1884
	George Dickson, M.D., F.R.C.S. Ed., 9 India Street,	1884
165	Thomas Wyld Pairman, L.R.C.P. & S. Ed., Te Awamutu, Waipa, Auckland, N.Z.	1884
	Alexander Thom, M.D., C.M., Crieff,	1884
	Hugh Logan Calder, M.D., C.M., 42 Leith Walk,	1884
	James Craig Balfour, L.R.C.P. & S. Ed., Redbourne, Kirkton-Lindsay, Lincolnshire,	1884
	Frederick Anastasius Saunders, L.R.C.P. & S. Ed., Denburn, Crail,	1884
170	Wm. Richardson, M.D., F.R.C.S. Ed. Bath Lodge, Reading,	1884
	Andrew Brown, M.D., M.R.C.P. Ed., 1 Bartholomew Road, Kentish Town, London, N.W.	1884
	G. J. H. Bell, M.B., C.M., Surgeon, Bengal Army,	1884
	T. Goodall Nasmyth, M.B., C.M., Cowdenbeath, Fife,	1884
	Henry Hay, M.B., C.M., 7 Brandon Street,	1884
175	Thomas R. Scott, M.D., C.M., Musselburgh,	1884
	R. Milne Murray, M.B., F.R.C.P. Ed., 10 Hope Street,	1884
	A. Murray Gibson, M.D., Portobello,	1884
	A. S. Cumming, M.D., F.R.C.P. Ed., 18 Ainslie Place,	1884
	Ernest F. Neve, M.D., F.R.C.S. Ed., M.R.C.S. Eng., Hospital, Srinagar, Kashmir, N.W. India,	1884
180	W. C. Greig, M.B., C.M., 69 Church Street, St Helens, Lancashire,	1884
	William Wilson, M.B., C.M., 2 N. Charlotte Street,	1885
	John Mowat, M.D., 1 Hope Park Terrace,	1885
	Skene Keith, M.B., F.R.C.S. Ed., 42 Charles Street, Berkeley Square, London, W.,	1885
	D. Noël Paton, M.D., F.R.C.P. Ed., 4 Walker Street,	1885
185	George Hugh Mackay, M.B., C.M., Elgin,	1885
	J. Michael Dewar, M.B., C.M., 24 Lauriston Place,	1885
	Edward M'Callum, F.R.C.S. Ed., 3 Brandon Street,	1885
	T. Edgar Underhill, M.D., F.R.C.S. Ed., Bromsgrove, Worcestershire,	1885
	John Struthers Stewart, L.R.C.P. & S. Ed., 16 Merchiston Terrace,	1885

		Date of Admission.
190	Allen Thomson Sloan, M.D., C.M., 22 Forth Street, . .	1885
	John William Ballantyne, M.B., M.R.C.P. Ed., 50 Queen St.,	1885
	James Robertson Crease, F.R.C.S. Ed., 2 Ogle Terrace, South Shields, ...	1885
	George Kerr, M.B., C.M., 9 Great Stuart Street, . .	1885
	Tom Bairstow, L.R.C.P. & S. Ed., 14 Buccleuch Place, .	1885
195	David Milligan, M.B., C.M., 7 West Maitland Street, .	1885
	George Dods, M.D., L.R.C.S. Ed., 50 Great King Street, .	1885
	J. Murdoch Brown, M.B., F.R.C.P. Ed., 9 Walker Street, .	1885
	Robert W. Felkin, M.D., F.R.C.S. Ed., 20 Alva Street, .	1885
	S. Hale Puckle, M.B., C.M., Bishop's Castle, Shropshire, .	1885
200	James Haig Ferguson, M.B., M.R.C.P. Ed., M.R.C.S. Eng., 16 Hope Street, ...	1885
	Charles Kennedy, M.D., C.M., 25 Newington Road, . .	1886
	William Gayton, M.D., M.R.C.S. Eng., Bartram Lodge, Fleet Road, Hampstead, London, N.W., ...	1886
	Reginald Ernest Horsley, M.B., C.M., 46 Heriot Row, .	1886
	James Mill, M.B., C.M., 178 Ferry Road, . . .	1886
205	Robert Fraser Calder Leith, M.B., B.Sc., 82 Marchmont Crescent, ...	1886
	Thomas M. Burn-Murdoch, M.B., C.M., 31 Morningside Road,	1886
	Professor William Smith Greenfield, M.D., F.R.C.P. Lond. and Ed., 7 Heriot Row, ...	1886
	Oswald Gillespie Wood, M.D., F.R.C.S. Ed., Surgeon, Army Medical Staff. India, ...	1886
	James Hogarth Pringle, M.B., C.M., 5 Livingstone Place, .	1886
210	Nathaniel Thomas Brewis, M.B., F.R.C.P. Ed., 59 Queen Street,	1886
	John Batty Tuke, jr., M.B., M.R.C.P. Ed., Balgreen, Murrayfield, ...	1886
	David Berry Hart, M.D., F.R.C.P. Ed., 29 Charlotte Square,	1886
	Walter Scott Lang, M.D., F.R.C.S. Ed., M.R.C.S. Eng., 1 Leopold Place, ...	1886
	Alfred Bell Whitton, M.B., C.M., Aberchirder, . .	1886
215	Robert S. Aitchison, M.B., M.R.C.P. Ed., 74 Great King Street, ...	1887
	J. A. Armitage, M.B., C.M., 15 Waterloo Road, Wolverhampton,	1887
	J. Walton Hamp, L.F.P.S. Glasg., L.S.A. Lond., Wolverhampton, ...	1887
	William Hunter, M.D., M.R.C.S. Eng., 16 Panton Street, Cambridge, ...	1887
	Sydney Rumboll, L.R.C.P. and S. Ed., Grangemouth, .	1887
220	John Thomson, M.B., F.R.C.P. Ed., 14 Coates Crescent, .	1887
	George Franklin Shiels, M.D., C.M., San Francisco, . .	1887
	T. Brown Darling, M.B., C.M., 36 South Bruntsfield Place, .	1887
	John Keay, M.B., C.M., Mavisbank House, Polton, . .	1887
	John F. Sturrock, M.B., C.M., Golspie, . . .	1887
225	Edward Carmichael, M.D., 8 Mansfield Place, . .	1887
	Charles C. Teacher, M.B., C.M., 5 Newington Road, . .	1887
	David W. Aitken, M.B., C.M., 3 Argyle Place, . .	1887
	Robert Inch, M.B., C.M., Gorebridge, ...	1887
	John Shaw M'Laren, M.B., F.R.C.S. Ed., 14 Walker Street, .	1887
230	George Mackay, M.B., F.R.C.S. Ed., M.R.C.S. Eng., 2 Randolph Place, ...	1887
	Henry Alexis Thomson, M.B., C.M., 6A Bruntsfield Place, .	1887
	David Wallace, M.B., C.M., 66 Northumberland Street, .	1887
	John C. Messer, M.D., R.N., 1 Lansdowne Crescent, . .	1887
	Thomas W. Dewar, M.B., M.R.C.P. Ed., 4 Stafford Street, .	1887
235	D. H. Anderson, M.B., C.M., Borough Asylum, Hull, .	1887
	James Lockhart Wilson, M.B., C.M., Duns, . . .	1888
	William Booth, F.R.C.S. Ed., 2 Minto Street, . .	1888
	John M'Fadyean, M.B., C.M., 9 East Hermitage Place, Leith, ...	1888

		Date of Admission.
	Thomas Russell, L.F.P.S. Glasg., Davidson's Mains, . .	1888
240	T. Home Ross, M.B., C.M., 40 York Place, . . .	1888
	George M. Johnston, M.D., C.M., 9 Morton Street, Leith, .	1888

NON-RESIDENT.

	Arthur Edward Turnour, M.D., M.R.C.S. Eng., *Denbigh*, .	1843
	W. Ord M'Kenzie, M.D., L.R.C.S. Ed., *London*, . .	1845
	W. Judson Van Someren, M.D., L.R.C.S. Ed., *Redhill, Surrey*,	1845
245	William H. Lowe, M.D., F.R.C.P. Ed., *Wimbledon*, . .	1845
	George Skene Keith, M.D., F.R.C.P. Ed., *Currie*, . .	1845
	Veitch Sinclair, L.R.C.P. and S. Ed., *London*, . .	1850
	Andrew Graham, M.D., Fleet Surgeon, R.N., . . .	1853
	Archibald Hall, M.D., *Montreal*, ...	1853
250	John Traill, F.R.C.S. Ed., *Arbroath*, ...	1853
	W. Overend Priestly, M.D., LL.D., F.R.C.P. Ed., *London*, .	1854
	Horatio Robinson Storer, M.D., *Newport, Rhode Island, U.S.*,	1855
	James C. Howden, M.D., *Montrose*, ...	1856
	Thomas Skinner, M.D., L.R.C.S. Ed., *London*, . .	1856
255	Professor William Smoult Playfair, M.D., LL.D., F.R.C.P.L., *London*, ...	1857
	J. Ivor Murray, M.D., F.R.C.S. Ed., *Scarboro'*, . .	1857
	Andrew Scott Myrtle, M.D., L.R.C.S. Ed., *Harrogate*, .	1859
	Robert Foulis, M.D., F.R.C.S. Ed., *Cupar-Fife*, . .	1859
	Francis Robertson Macdonald, M.D., *Inveraray*, . .	1860
260	Professor John Young, M.D., *University of Glasgow*, . .	1860
	Norman Bethune, M.D., F.R.C.S. Ed., *Toronto*, . .	1861
	George Thin, M.D., L.R.C.S. Ed., *London*, . . .	1861
	Peter Gordon, L.R.C.P. and S. Ed., *Juniper Green*, . .	1861
	J. Cecil Phillippo, M.D., *Kingston, Jamaica*, . . .	1861
265	Professor William Stephenson, M.D., F.R.C.S. Ed., *Aberdeen*,	1861
	David Yellowlees, M.D., LL.D., F.F.P.S. Glasg., *Glasgow*, .	1862
	William M'Cullock Watson, M.D., *Montrose*, . . .	1863
	Prof. Arthur Gamgee, M.D., F.R.C.P. Ed., F.R.S , *St Leonards-on-Sea*, ...	1864
	Professor John Cleland, M.D., LL.D., *The University, Glasgow*,	1864
270	R. B. Finlay, M.D., M.P., *Middle Temple, London*, . .	1864
	Stanley Lewis Haynes, M.D., M.R.C.S. Eng., *Malvern*, .	1864
	Francis D. A. Skae, M.D., *Lerwick*, ...	1864
	James Watt Black, M.D., F.R.C.P.L., *London*, . .	1865
	David Brodie, M.D., *Canterbury*, ...	1865
275	Thomas Sheriff, L.R.C.P. and S. Ed., *Edinburgh*, . .	1867
	Peter Maury Deas, M.B., L.R.C.S. Ed., *Exeter*, . .	1868
	Professor J. G. M'Kendrick, M.D., F.R.C.P. Ed., *University, Glasgow*, ...	1870
	Lawson Tait, M.D., F.R.C.S. Ed. and Eng., *Birmingham*, .	1870
	J. G. Sinclair Coghill, M.D., F.R.C.P. Ed., *Ventnor*, . .	1870
280	James Johnston, M.D., F.R.C.S. Ed., *London*, . .	1871
	J. William Eastwood, M.D., M.R.C.P.L., *Darlington*. .	1871
	Professor J. Bell Pettigrew, M.D., LL.D., F.R.C.P. Ed., *University of St Andrews*, ...	1873
	John Aymers Macdougall, M.D., F.R.C.S. Ed., *Carlisle*, .	1875
	Thomas John Maclagan, M.D., M.R.C.P.L., *London*, . .	1875
285	Dr Groesbeck, *Cincinnati*, ...	1875
	Professor David James Hamilton, M.B., F.R.C.S. Ed., *Aberdeen University*, ...	1876
	J. Moolman, M.B., C.M., *Cape of Good Hope*, . . .	1877
	Robert Somerville, M.D., L.R.C.S. Ed., *Galashiels*, . .	1877
	Graham Steell, M.D., M.R.C.P.L., *Manchester*, . .	1877
290	Frederick William Barry, M.D., D.Sc., *London*, . .	1878
	Thomas Inglis, F.R.C.P. Ed., *Lincoln*, ...	1878
	John Brown, M.D., F.R.C.S. Eng., *Burnley*, . . .	1878

		Date of Admission.
	Walter Weir, M.B., F.R.C.P. Ed., *London*,	1879
	Keith Norman Macdonald, M.D., F.R.C.P. Ed., *Cupar-Fife*,	1880
295	John Home Hay, M.D., M.R.C.S. Eng., *Alloa*,	1880
	John Mackay, M.D., L.R.C.S. Ed., *Aberfeldy*,	1881

ORDINARY MEMBERS

ARRANGED ALPHABETICALLY.

RESIDENT.

	Dr J. O. Affleck, 38 Heriot Row,	1871
	Dr R. S. Aitchison, 74 Great King Street,	1887
	Dr D. Aitken, 3 Argyle Place,	1887
	Dr D. H. Anderson, Borough Asylum, Hull,	1887
5	Dr James Andrew, 2 Atholl Crescent,	1869
	Professor Annandale, 34 Charlotte Square,	1863
	Dr Archibald, Woodhouse-Eaves, Loughborough,	1882
	Dr J. A. Armitage, 15 Waterloo Road, Wolverhampton,	1887
	Dr W. Badger, Penicuik,	1882
10	Dr J. Johnson Bailey, Marple, Cheshire,	1874
	Dr Edwin Baily, Oban,	1883
	Tom Bairstow, Esq., 14 Buccleuch Place,	1885
	Dr Andrew Balfour, Portobello,	1874
	Dr J. H. Balfour, Portobello,	1881
15	Dr G. W. Balfour, 17 Walker Street,	1874
	Dr James Craig Balfour, Redbourne, Kirkton-Lindsay, Lincolnshire,	1884
	Dr Thomas Balfour, 51 George Square,	1856
	Dr Alexander Ballantyne, Dalkeith,	1872
	Dr J. W. Ballantyne, 50 Queen Street,	1885
20	Dr A. H. Freeland Barbour, 24 Melville Street,	1881
	Dr G. J. H. Bell, Surgeon, Bengal Army,	1884
	Joseph Bell, Esq., 2 Melville Crescent,	1862
	G. H. Bentley, Esq., Kirkliston,	1877
	Dr G. A. Berry, 23 Rutland Street,	1883
25	Dr James S. Beveridge, 8 Eildon Street,	1861
	Dr Alexander Black, 8 St Vincent Street,	1883
	Dr W. T. Black, 2 George Square,	1877
	Dr Robert H. Blaikie, 9 Palmerston Road,	1883
	Dr Bleloch, 2 Lonsdale Terrace,	1871
30	William Booth, Esq., 2 Minto Street,	1888
	Dr Brakenridge, 10 St Colme Street,	1865
	Dr Byrom Bramwell, 23 Drumsheugh Gardens,	1876
	Dr N. T. Brewis, 59 Queen Street,	1886
	Dr Brown, 1 Bartholomew Road, Kentish Town, London, N.W.,	1884
35	Dr J. Graham Brown, 16 Ainslie Place,	1878
	Dr J. Macdonald Brown, 12 South Mansionhouse Road,	1883
	Dr J. Murdoch Brown, 9 Walker Street,	1885
	Dr Alexander Bruce, 13 Alva Street,	1883
	Dr Robert Bruce, 12 York Place,	1858
40	Dr Buist, 1 Clifton Terrace,	1877
	Dr T. M. Burn-Murdoch, 31 Morningside Road,	1886
	Dr Cadell, 5 Castle Terrace,	1870
	Dr Francis M. Caird, 21 Rutland Street,	1883
	Dr H. L. Calder, 42 Leith Walk,	1884
45	Dr W. Watson Campbell, Duns,	1877
	Dr Cappie, 37 Lauriston Place,	1855
	Dr Edward Carmichael, 8 Mansfield Place,	1887
	Dr J. Carmichael, 22 Northumberland Street,	1870

		Date of Admission.
	Dr C. W. Cathcart, 8 Randolph Crescent, *Secretary*,	1883
50	Dr T. F. S. Caverhill, 8A Abercromby Place,	1884
	Professor John Chiene, 26 Charlotte Square, *Vice-President*,	1867
	Dr Church, 36 George Square,	1876
	Dr Clouston, Tipperlinn House, Morningside Place, *Vice-President*,	1861
	Dr A. R. Coldstream, Florence, Italy,	1878
55	Dr John Connel, Peebles,	1876
	Dr Cotterill, 23 Walker Street,	1878
	Dr William Craig, 7 Bruntsfield Place,	1869
	Dr J. R. Crease, 2 Ogle Terrace, South Shields,	1885
	Dr Halliday Croom, 25 Charlotte Square,	1870
60	Dr A. S. Cumming, 18 Ainslie Place,	1884
	Dr R. J. B. Cunynghame, 6 Walker Street,	1868
	Dr J. B. Darling, 36 South Bruntsfield Place,	1887
	Dr J. M. Dewar, 24 Lauriston Place,	1885
	Dr T. W. Dewar, 4 Stafford Street,	1887
65	Dr Archibald Dickson. 11 Royal Circus,	1871
	Dr George Dickson, 9 India Street,	1884
	Dr George Dods, 50 Great King Street,	1885
	Dr Halliday Douglas, 30 Melville Street,	1842
	Dr William B. Dow, Dunfermline,	1879
70	Dr John Duncan, 8 Ainslie Place,	1868
	Dr Kirk Duncanson, 22 Drumsheugh Gardens,	1871
	Dr H. M. Dunlop, 20 Abercromby Place,	1883
	Dr J. Dunsmure, 53 Queen Street,	1872
	C. H. Fasson, Esq., Dep. Surg.-Gen., Royal Infirmary,	1879
75	Dr R. W. Felkin, 20 Alva Street,	1885
	Dr J. Haig Ferguson, 16 Hope Street,	1885
	Dr W. A. Finlay, St Helen's, Russell Place, Trinity,	1875
	Dr Andrew Fleming, 8 Napier Road,	1880
	Dr Foulis, 34 Heriot Row,	1875
80	Dr F. W. Dyce Fraser, South Lodge, Ascot, Berks,	1883
	Dr John Fraser, 19 Strathearn Road,	1878
	Professor Thomas R. Fraser, 13 Drumsheugh Gardens,	1865
	Dr R. Freeland, Broxburn,	1879
	Dr Garland, 35 Charlotte Street, Leith,	1873
85	Dr W. Gayton, Bartram Lodge, Fleet Road, Hampstead, London, N.W.,	1886
	Dr A. Murray Gibson, Portobello,	1884
	Dr G. A. Gibson, 17 Alva Street,	1880
	Dr James D. Gillespie, 10 Walker Street,	1852
	G. R. Gilruth, Esq., 48 Northumberland Street,	1869
90	Dr J. Allan Gray, 107 Ferry Road,	1879
	Professor Greenfield, 7 Heriot Row,	1886
	Dr David Greig, 38 Coates Gardens,	1854
	Dr W. C. Greig, 69 Church Street, St Helen's, Lancashire,	1884
	Dr R. H. Gunning, 12 Addison Crescent, West Kensington, London, W.,	1846
95	Dr John Haddon, Melrose,	1883
	Dr J. W. Hamp, Wolverhampton,	1887
	Professor A. W. Hare, Owens College, Manchester,	1883
	Dr D. Berry Hart, 29 Charlotte Square,	1886
	Dr Henry Hay, 7 Brandon Street,	1884
100	Dr John Henderson, 7 John's Place, Leith,	1848
	Dr J. W. B. Hodsdon, 30 Walker Street,	1883
	Dr R. E. Horsley, 46 Heriot Row,	1886
	Dr George Hunter, Linlithgow,	1876
	Dr James A. Hunter, 18 Abercromby Place,	1851
105	Dr W. Hunter, 16 Panton Street, Cambridge,	1887
	Dr Husband, 28 Clarence Street,	1849
	Francis B. Imlach, Esq., 48 Queen Street,	1843

		Date of Admission.
	Dr Robert Inch, Gorebridge,	1887
	Dr Archibald Inglis, 33 Albany Street,	1827
110	Dr W. Wotherspoon Ireland, Prestonpans,	1883
	Dr James, 44 Melville Street,	1877
	Dr W. Allan Jamieson, 26 Rutland Street,	1876
	Dr James Jamieson, 43 George Square,	1877
	Dr G. M. Johnston, 9 Morton Street, Leith,	1888
115	Dr R. M'Kenzie Johnston, 5 Rutland Square,	1883
	Dr J. Carlyle Johnstone, Melrose Asylum, Melrose,	1882
	Dr J. Keay, Mavisbank House, Polton,	1887
	Dr Keiller, 21 Queen Street,	1845
	Dr Skene Keith, 42 Charles Street, Berkeley Square, London, W.,	1885
120	Dr Thomas Keith, 42 Charles Street, Berkeley Square, London, W.,	1852
	Dr C. Kennedy, 25 Newington Road,	1886
	Dr George Kerr, 9 Great Stuart Street,	1885
	Dr W. Scott Lang, 1 Leopold Place,	1886
	Dr Robert Lawson, 24 Mayfield Terrace,	1881
125	Dr R. F. C. Leith, 82 Marchmont Crescent,	1886
	Dr George Leslie, Falkirk,	1881
	Dr Linton, 60 George Square,	1863
	Dr Littlejohn, 24 Royal Circus,	1853
	Dr Lucas, Dalkeith,	1875
130	Dr R. A. Lundie, 35 Warrender Park Road,	1883
	Dr P. M'Bride, 16 Chester Street,	1879
	Dr E. M'Callum, 3 Brandon Street,	1885
	Dr J. M'Fadyean, 9 East Hermitage Place, Leith,	1888
	John M'Gibbon, Esq., 55 Queen Street,	1868
135	Dr MacGillivray, 11 Rutland Street,	1877
	Dr G. Mackay, 2A Gilmore Place,	1878
	Dr George Mackay, 2 Randolph Place,	1887
	Dr G. H. Mackay, Elgin,	1885
	Professor Sir Douglas Maclagan, 28 Heriot Row,	1834
140	Dr J. S. M'Laren, 14 Walker Street,	1887
	Dr P. H. Maclaren, 1 Drumsheugh Gardens,	1868
	Dr Roderick M'Laren, 23 Portland Square, Carlisle,	1882
	Dr Donald MacRaild, Greenock,	1883
	Dr A. Matthew, Corstorphine,	1882
145	Dr D. Menzies, 21 Rutland Square,	1878
	Dr W. Menzies, 115 Lothian Road,	1847
	Dr J. C. Messer, 1 Lansdowne Crescent,	1887
	Dr J. Mill, 178 Ferry Road,	1886
	A. G. Miller, Esq., 7 Coates Crescent, *Treasurer*,	1867
150	Dr D. Milligan, 7 West Maitland Street,	1885
	Sir Arthur Mitchell, 34 Drummond Place,	1859
	Dr Moir, 52 Castle Street,	1836
	Dr Alexander Moir, 30 Buccleuch Place,	1876
	Dr J. Rutherford Morison, 14 Saville Row, Newcastle-on-Tyne,	1882
155	Dr John Mowat, 1 Hope Park Terrace,	1885
	Dr Claud Muirhead, 30 Charlotte Square,	1866
	Dr R. Milne Murray, 10 Hope Street,	1884
	Dr A. D. Leith Napier, 3 Beaufort Gardens, London,	1879
	Dr T. Goodall Nasmyth, Cowdenbeath, Fife,	1884
160	Dr E. F. Neve, Dispensary, Srinagar, Kashmir, N.W. India,	1884
	Dr H. Newcombe, 5 Dalrymple Crescent,	1883
	Dr P. Orphoot, 113 George Street,	1865
	Dr T. W. Pairman, Te Awamutu, Waipa, Auckland, N.Z.,	1884
	Dr Paterson, 4 Coates Crescent,	1847
165	Dr D. Noël Paton, 4 Walker Street,	1885
	Dr Peddie, 15 Rutland Street,	1842

		Date of Admission.
	Dr J. A. Philip, Rue Victor Hugo, Boulogne-Sur-Mer,	1878
	Dr R. W. Philip, 4 Melville Crescent,	1883
	Dr Playfair, 5 Melville Crescent,	1874
170	Dr J. H. Pringle, 5 Livingstone Place,	1886
	Dr S. Hale Puckle, Bishop Castle, Shropshire,	1885
	Dr Rattray, Portobello,	1874
	Dr William Richardson, Bath Lodge, Reading,	1884
	Dr James Ritchie, 14 Charlotte Square, *Secretary*,	1873
175	Dr R. Peel Ritchie, 1 Melville Crescent, *Vice-President*,	1862
	Dr Argyll Robertson, 18 Charlotte Square,	1861
	Dr Ronaldson, 18 Bruntsfield Place,	1877
	Dr J. Maxwell Ross, 112 Gilmore Place,	1882
	Dr Joseph C. Ross, Penzance,	1884
180	Dr T. Home Ross, 40 York Place,	1888
	Dr S. Rumboll, Grangemouth,	1887
	Thomas Russell, Esq., Davidson's Mains,	1888
	Dr William Russell, 46 Albany Street,	1884
	Professor Rutherford, 14 Douglas Crescent,	1866
185	Dr F. A. Saunders, Denburn, Crail,	1884
	Dr Thomas R. Scott, Musselburgh,	1884
	Dr Andrew Semple, 10 Forres Street,	1883
	Dr John Shand, 34 Albany Street,	1878
	C. H. E. Sheaf, Esq., Toowoomba, Queensland, Australia,	1871
190	Dr G. F. Shiels, San Francisco,	1887
	Dr W. H. Shirreff, Melbourne, Australia,	1883
	Dr J. Sibbald, 3 St Margaret's Road,	1859
	Professor Simpson, 52 Queen Street,	1859
	Dr A. J. Sinclair, 21 Northumberland Street,	1873
195	Dr A. T. Sloan, 22 Forth Street,	1885
	Dr Andrew Smart, 20 Charlotte Square,	1865
	Dr G. D. Smith, 146 Ferry Road,	1877
	Dr John Smith, 11 Wemyss Place, *President*,	1856
	Professor Grainger Stewart, 19 Charlotte Square,	1861
200	Dr J. S. Stewart, 16 Merchiston Terrace,	1885
	Dr W. Stewart, Kirkwall,	1879
	Dr John Strachan, Dollar,	1867
	Dr James Struthers, 39 Charlotte Street, Leith,	1849
	Dr J. F. Sturrock, Golspie,	1887
205	Dr Johnson Symington, 2 Greenhill Park,	1878
	Dr W. Taylor, 12 Melville Street,	1871
	Dr C. C. Teacher, 5 Newington Road,	1887
	Dr C. H. Thatcher, 13 Albany Street,	1876
	Dr Alexander Thom, Crieff,	1884
210	Dr Alexander Thomson, 14 Rankeillor Street,	1849
	Dr H. A. Thomson, 6A Bruntsfield Place,	1887
	Dr John Thomson, 14 Coates Crescent,	1887
	Dr J. Stitt Thomson, Dalkeith,	1877
	Dr Francis Troup, 1 Minto Street,	1883
215	Dr Batty Tuke, 20 Charlotte Square,	1864
	Dr J. Batty Tuke, jr., Balgreen, Murrayfield,	1886
	Professor Sir William Turner, 6 Eton Terrace,	1858
	Dr R. S. Turner, Keith,	1867
	Dr Underhill, 8 Coates Crescent,	1872
220	Dr T. Edgar Underhill, Broomsgrove, Worcestershire,	1885
	Dr D. Wallace, 66 Northumberland Street,	1887
	B. C. Waller, Esq., M.B., Cowen-Bridge, Kirkby-Lonsdale,	1877
	Dr P. H. Watson, 16 Charlotte Square,	1856
	Dr W. Watson, Midcalder,	1862
225	Dr A. D. Webster, 20 Newington Road,	1883
	Dr Graham Weir, 36 Heriot Row,	1843
	Dr A. B. Whitton, Aberchirder,	1886
	Dr David Wilson, 12 Dean Terrace,	1844

		Date of Admission.
	Dr J. Lockhart Wilson, Duns,	1888
230	J. L. Wilson, Esq., 4 Buccleuch Place,	1883
	Dr T. D. Wilson, 10 Newington Road,	1880
	Dr William Wilson, 2 North Charlotte Street,	1885
	Dr Oswald G. Wood, India,	1886
	Dr Russell E. Wood, 9 Darnaway Street,	1883
235	Dr G. Sims Woodhead, 6 Marchhall Crescent,	1883
	Dr Strethill Wright, 8 St Aidan's Terrace, Claughton, Birkenhead,	1871
	Dr Hamilton Wylie, 1 George Place,	1883
	Dr John Wyllie, 1 Melville Street,	1868
	Dr James Young, 14 Ainslie Place,	1859
240	Dr P. A. Young, 25 Manor Place,	1870
	Dr Ziegler, 47 George Square,	1876

NON-RESIDENT.

	Dr F. W. Barry, *London*,	1878
	Dr Bethune, *Toronto*,	1861
	Dr J. W. Black, *London*,	1865
245	Dr Brodie, *Canterbury*,	1865
	Dr John Brown, *Burnley*,	1878
	Professor Cleland, *Glasgow*,	1864
	Dr Coghill, *Ventnor*,	1870
	Dr P. M. Deas, *Exeter*,	1868
250	Dr J. W. Eastwood, *Darlington*,	1871
	Dr R. B. Finlay, M.P., *Middle Temple, London*,	1864
	Dr Foulis, *Cupar-Fife*,	1859
	Professor Gamgee, *St-Leonards-on-Sea*,	1863
	Peter Gordon, Esq., *Juniper Green*,	1861
255	Dr A. Graham, R.N.,	1853
	Dr Groesbeck, *Cincinnati*,	1875
	Dr Archibald Hall, *Montreal*,	1853
	Professor D. J. Hamilton, *Aberdeen University*,	1876
	Dr J. H. Hay, *Alloa*,	1880
260	Dr Stanley Haynes, *Malvern*,	1864
	Dr J. S. Howden, *Montrose*,	1856
	Dr T. Inglis, *Lincoln*,	1878
	Dr James Johnston, *London*,	1871
	Dr George Keith, *Currie*,	1845
265	Dr Lowe, *Wimbledon*,	1845
	Dr F. R. Macdonald, *Inveraray*,	1860
	Dr K. N. Macdonald, *Cupar-Fife*,	1880
	Dr John A. Macdougall, *Carlisle*,	1875
	Dr John Mackay, *Aberfeldy*,	1881
270	Professor M'Kendrick, *Glasgow*,	1870
	Dr W. O. Mackenzie, D.I.G.H., *London*,	1845
	Dr T. J. Maclagan, *London*,	1875
	Dr J. Moolman, *Cape of Good Hope*,	1877
	Dr J. Ivor Murray, *Scarboro'*,	1857
275	Dr Andrew Myrtle, *Harrogate*,	1859
	Professor Bell Pettigrew, *St Andrews*,	1873
	Dr Phillippo, *Kingston, Jamaica*,	1860
	Professor W. S. Playfair, *London*,	1857
	Dr Priestley, *London*,	1854
280	Thomas Sheriff, Esq., *Edinburgh*,	1867
	Dr Sinclair, *London*,	1850
	Dr Francis Skae, *Lerwick*,	1864
	Dr T. Skinner, *London*,	1856
	Dr Van Someren, *Redhill, Surrey*,	1845

		Date of Admission.
285	Dr Somerville, *Galashiels*,	1877
	Dr Graham Steell, *Manchester*,	1877
	Professor Stephenson, *Aberdeen*,	1861
	Dr H. R. Storer, *Newport, Rhode Island, U.S.*,	1855
	Dr Lawson Tait, *Birmingham*,	1870
290	Dr Thin, *London*,	1861
	John Traill, Esq., *Arbroath*,	1853
	Dr Turnour, *Denbigh*,	1843
	Dr W. Watson, *Montrose*,	1863
	Dr Walter Weir, *London*,	1879
295	Dr Yellowlees, *Gartnavel Asylum, Glasgow*,	1862
	Professor John Young, *Glasgow*,	1860

N.B.—*Members are requested to communicate with the Secretaries if they discover any errors or omissions in the List, and also to intimate all changes in their addresses.*

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V.—SPECIAL DEMONSTRATIONS.

TRANSACTIONS

OF

THE MEDICO-CHIRURGICAL SOCIETY OF EDINBURGH,

FOR SESSION LXVII., 1887-88.

Meeting I.—November 2, 1887.

Professor Grainger Stewart, *President, in the Chair.*

I. Election of Office-Bearers.

The following gentlemen were appointed office-bearers for the ensuing session:—*President*, Dr John Smith; *Vice-Presidents*, Dr Peel Ritchie, Prof. Chiene, Dr Clouston; *Councillors*, Dr James, Dr MacGillivray, Dr Duddingston Wilson, Dr G. A. Gibson, Prof. Grainger Stewart, Dr J. Connel, Dr George Leslie, Dr Maxwell Ross; *Treasurer*, Mr A. G. Miller; *Secretaries*, Mr Charles W. Cathcart, 8 Randolph Crescent, and Dr James Ritchie, 14 Charlotte Square; *Editor of Transactions*, Dr William Craig, 7 Bruntsfield Place.

II. Election of Ordinary Members.

The following gentlemen were elected ordinary members of the Society:—David W. Aitken, M.B., C.M.; Robert Inch, M.B., C.M.; John Shaw M'Laren, M.B., C.M.; George Mackay, M.B., C.M.; H. A. Thomson, M.B., C.M., M.R.C.S. Eng.

III. Exhibition of Patients.

1. *Dr Byrom Bramwell* showed a man suffering from peculiar symptoms, the result of lead poisoning. He presented himself at the Infirmary a fortnight ago, complaining of dimness of vision,

severe headache, and tremor affecting particularly the right hand, but distributed generally over the body. The acuity of vision was found on examination to be reduced to less than one-tenth, and the fields very much restricted. Greens and blues could not be distinguished, and the colour fields were much reduced. There was no optic neuritis. The fundus was, in fact, perfectly normal—a very interesting, and (so far as Dr Bramwell knew) a very rare condition in cases of "lead blindness." Under large doses of iodide of potassium and sulphate of magnesia purgatives, the headache had disappeared, and vision was now normal.

2. *Mr C. W. Cathcart* showed two cases of PARTIAL FOOT AMPUTATION. The first was a railway porter, on whom a Chopart's amputation had been performed by Dr Shaw M'Laren in Chalmers's Hospital, about a year ago, for an injury to his foot, sustained in falling between the platform and a train. He was able to walk with ease and comfort, and showed only a slight limp in walking. The second was a young man who had had a double amputation performed by Mr Duncan two years ago. At the left ankle a Syme had been done, and in the right foot a Lisfranc. The right boot appeared to be the more ugly of the two, but the patient declared that the right was the better stump for walking on.

IV. VALEDICTORY ADDRESS.

By T. GRAINGER STEWART, M.D., F.R.C.P. Ed., Professor of the Practice of Physic in the University of Edinburgh; Physician in Ordinary to the Queen for Scotland, etc.

GENTLEMEN,—In accordance with the custom of the Society, I rise to address you for a few minutes before I relinquish the Presidentship to which two years ago you did me the honour of electing me. Permit me again, as on that occasion, to thank you cordially for the honour, and now, also, for the courtesy and consideration which I have invariably experienced, and to assure you that I reckon it among the greatest distinctions which the profession in Edinburgh could confer, that one should be called to preside over this distinguished Society.

On considering the events which have occurred during these two years, we cannot fail to be impressed with the number and formidable character of the losses which we have sustained by death. We have lost the last personal link that bound us to the first year of the Society's existence, for it was in 1821, when the Society was newly formed, when Dr Duncan, senior, was President, that Mr William Brown became a member. There may be some here who knew little or perhaps nothing of the genial, God-fearing, quaint, and kindly man, who so quietly and unostentatiously performed his duties to his patients and his College, to this Society, and to the Medical Missionary Society over which he presided for so many years. By those of us who did know him his memory must always be treasured as that of a man who most faithfully fulfilled every duty that was entrusted to him. Nor can those of us who are passing into the ranks of the seniors forget the noble qualities and conscientious laboriousness of Dr Burns, the geniality and professional skill of Dr Dunsmure, nor the strong character, wide reading, and eminent culture of Dr Cumming. I shall not dwell upon the valuable qualities of Dr Finlay of Newhaven, Dr Williamson of Leith, or Mr Pridie of Newington, nor of our esteemed country brethren, Drs Longmuir of Bathgate and Fergusson of Peebles, nor of men cut off early in life like Dr Francis Moinet, and Dr Bennet of Leith, whose recent death his friends so deeply deplore. But I must allow myself one word regarding Dr James Sidey, a man endeared to the community by many fine qualities. The son of a shrewd and indeed very able practitioner, he was early in life introduced to a large clientèle, and from the beginning to the end of his career he conducted with the utmost assiduity his arduous practice. I have heard it asserted that the cabmen on night duty were more familiar with his address than with that of any other doctor in the New Town; and that one of them had been known to assure an anxious father that he had already twice been down for the Doctor in that single night. In his practice he was ingenious and often original, and his resources were rich beyond those of most. But although working so hard he found time for amusements, and for the enjoyment of the artistic and literary companionships by which his life was enriched. The genial, humorous, and happy songs and sketches of his *Mistura Curiosa* and *Alter Ejusdem*, made him known and liked beyond the immediate circle of his acquaintances, and the regret was sincere and widespread when it became known that he had succumbed to the sea of troubles which had arisen around him.

Dr Angus Macdonald's career was a typical illustration of one of the finest points in the history of our profession and of other professions in Scotland. I have heard people in his native district speak with pride of the bright, eager, capable boy, and I saw with what interest they had watched how he had succeeded in breasting the blows of circumstance and grappling with his evil star; how he made his way from school to college, and after a distinguished career as an undergraduate took his degree in Arts in the University of Aberdeen; how he came to Edinburgh, and there was equally distinguished as a student of medicine, and worked his way up till he enjoyed one of the foremost positions in the department of professional life to which he had devoted himself. The failure of his health caused widespread regret, and his early death was lamented by multitudes.

Dr Rutherford Haldane's career, although in some points resembling, differed in many particulars from that of Dr Macdonald. Descended from a line of ancestors illustrious in science, in medicine, in law, in divinity, and in war, he showed from early days that he was gifted with unusual ability. In the interesting memoirs of James and Robert Haldane it is told with what pleasure his father heard that the young student in his second year of medical study had taken the highest place in physiology and in surgery, distancing in the latter his fellows of the fourth year. His achievements in other studies and at his graduation corresponded to this success. His long tenure of the Pathologistship and of the Physicianship to the Royal Infirmary gave him a firm grasp of practical medicine, and the lucidity, I might say the luminousness, of his expositions made him one of the most popular lecturers in the School. I have heard those who knew him well speak with intense admiration of his diagnostic skill and practical sagacity, and it is probable that had he not possessed very ample means he would have acquired a much wider practice than he did. His unfailing courtesy, his extraordinary business capacity, and his faculty of lucid statement, to which I have already referred, combined to render him one of the most useful and esteemed members of this Society, as of the Royal College of Physicians, the General Medical Council, and the University Court.

There was one other member of the Society who died within the past two years, and who was better known to myself than to most of the members, and I crave your indulgence while I say a word of him which some of you may consider out of proportion to the services he was permitted to render the Society. From his earliest student days I was intimately acquainted with Dr John Bishop. While still engaged in business in his native town of Sheffield he was a constant student of literature and philosophy. He corresponded upon philosophic questions with Dean Mansell, and was familiar with the writings of Sir William Hamilton.[1] He worked also at chemistry and other departments of science, and whenever his circumstances made it possible he relinquished business in order to study medicine. Coming to Edinburgh, he became the devoted admirer of Goodsir and Turner, of Syme and Lister, and when he had graduated he became the private assistant of the author of the antiseptic system. Of the esteem in which he was held by his chief no better token could be given than the fact, that Sir Joseph volunteered to go from London to the Riviera, in the depth of winter, in order to try whether surgical operation might avert his impending death. The moral and intellectual qualities of Dr Bishop were of the highest order. His professional attainments and the zeal with which he discharged his duties deserve the utmost praise, and those of us who knew him best feel very deeply how much was lost to the profession by his early death. Like Mr Brown, of whom I spoke first, he was a man of

[1] His philosophic library is to form the nucleus of the library of reference of the University, kindly having been presented to that Institution by his widow.

deep religious earnestness, and exerted himself strenuously on behalf of medical missions and kindred institutions.

We cannot but be saddened when we contemplate the long list of formidable losses which the Society has sustained, and to which I have thus briefly referred.

On such an occasion as this it may be appropriate for us as members of this, the chief Medical Society of Edinburgh, and the Institution which most generally represents the brotherhood of the profession, to make a brief survey of the work that the profession is doing in the community, and consider in what respects and in what manner it might be better done.

In a great medical centre such as Edinburgh the profession performs at least four functions, and I should like to draw your attention briefly to each of them. Let us see what we are doing in our work as *practitioners*, what we are doing towards the *prevention of disease*, what we are doing in the way of *advancing medical knowledge*, and what in regard to the *education* of those who are to be our colleagues and successors.

It will not be disputed that our profession always does its work best when all these departments are in full operation. The whole tone of practice in a community is raised when a medical school becomes established, and it is when practice is at its best that the value of sanitary improvements is most fully realized, and original investigations are most likely to be made, appreciated, and encouraged. The community, as well as the profession in Edinburgh, may be congratulated upon the fact that here for so many years the profession has been discharging each of the four functions.

In Practice.

It is manifest that under the first of my four categories are embraced relationships of so special, personal, and private a nature that it is difficult to form an estimate of the way in which they are fulfilled, at least such an estimate as could be formulated on a public occasion like this; but I think we may, without arrogance, claim that our work as practitioners in Edinburgh is done as efficiently as it is in most other places, although, doubtless, each one of us is painfully conscious of his own errors in the past and deficiencies in the present.

But there are some questions which we can very easily discuss, and to which I shall ask your attention for a moment; and first, as to the proportion which our profession bears to the population now as compared with twenty-five years ago. I select that time, as I have myself been cognizant of what has been going on in the profession during that period. I find that in 1862 the population was about 169,000, and there were 156 medical practitioners in the city, or one for rather less than each 1100 of the inhabitants. In 1886, the population had risen above 211,000, and the number of medical men was 224, or one to rather more than 900 of the population. Thus the community is abundantly provided with medical men, and the cry is still they come. Many hold that the profession here is becoming overstocked, and certainly the competition has no lack of keenness, but the character of the population in Edinburgh justifies a larger proportion of doctors than would be warranted in the case of a manufacturing town, and the numerous positions in connexion with our Medical School induces capable and ambitious young men to settle in the city. It appears also that during the twenty-five years the status of the profession has in some respects improved, for whereas out of the 156 practitioners in 1862, 11, or one in 14, had druggists' shops, now only 3 out of the 224, or one in 74, are in the same position. This change has followed upon another of which I have often heard my older professional brethren speak, namely, the disappearance of the custom of practitioners dispensing their own drugs, a custom which still persists extensively in England, but disappeared from Edinburgh fifty or sixty years ago.

Our medical institutions have during the twenty-five years undergone a wonderful development. In 1862 we had the Royal Infirmary, the Sick Children's Hospital, the Maternity Hospital, and the Royal Asylum for the Insane. We now have these, and in addition we have the Chalmers Hospital, the City Fever Hospital, the Longmore Hospital for Incurables, and our old institutions have immensely increased in extent. Whereas the Royal Infirmary received in 1862, 3892 patients, last year it received 8088, while its expenditure has risen from £14,000 to £33,000 a year. The magnificent buildings, of which the city is so justly proud, have replaced those which, although admirable at the time they were built, had fallen far behind modern ideas of hospital equipment.

The Sick Children's Hospital was in its infancy in 1862, for it then had 23 beds, and the number of children received during the year was 265. It now has 72 available beds, and last year received 649 patients. It is to be noted also that both in the Infirmary and the Sick Children's Hospital beds which were formerly set aside for fever cases are no longer required since the transference of the charge of fever cases to the City Hospital.

The Old Maternity Hospital appears to have received in 1862 as many in-patients as the present one does, but the accommodation now provided is very different, and the number of patients attended at their own homes is much greater.

The Royal Asylum for the Insane afforded accommodation in 1862 for 680, now for 825. The number of patients has risen from 679 to 803. The number of the staff has been increased from 118 to 182, and the expenditure from less than £23,000 to about £39,000 a year.

The City Fever Hospital is arranged so as to meet the requirements of any ordinary epidemic. During the late outbreak of scarlet fever as many as 270 cases were housed there at one time, and more could have been admitted, and from intimate acquaintance with the arrangements of the Hospital, I can testify that it is most admirably organized.

The Longmore Hospital affords accommodation for 66 cases which are regarded as incurable, and in 1886 it had in all 108 cases under treatment.

Besides all these institutions in which resident patients are treated, we have a large and increasing number of dispensaries in vigorous operation, supplying advice and medicine to the poorer classes in the most liberal way. I think, gentlemen, that we are entitled to claim that on the whole the community is excellently served.

As to the position enjoyed by the members of our profession, it also is satisfactory. The public knows well the value of really good medical advice, and accords to the profession a most generous recognition. It may be held to be better than it is in some more southerly regions, where the professional emoluments may be greater.

In Prevention.

Our second question relates to what the profession is accomplishing in the way of sanitation and the prevention of disease. It is scarcely necessary to vindicate the expression "the profession is accomplishing," for every one who is familiar with the subject will admit that it is to our profession that the community is mainly indebted for the advances made in these respects, and I cannot but say that in a very special measure Edinburgh is indebted to my predecessor in this chair, the Officer of Public Health. The population of the city in 1862 was 170,000, the deaths that year were 4661; in 1886 the population had risen to 211,406, while the deaths had fallen to 4149. Thus in 1862 the death-rate was 26·65 per thousand; in 1886 it had fallen to 19·62. Such a change is eminently satisfactory, but one is led naturally to turn to the statistics of the zymotic mortality. In 1862 the zymotic group accounted for 19·73 per cent. of the total deaths, in 1886 for 8·34 per cent. only, and I am assured that this is not merely an accidental result of epidemic outbreak in 1862, or immunity from this in 1886, but corresponds to continuous experience. It is thus manifest that the improvement has occurred mainly in the groups of diseases most influenced by sanitary precautions.

The diminution of mortality implies an immense saving of life, and when we consider that each fatal illness is held to correspond to twelve serious illnesses not terminating fatally, we see what an immense saving to the wealth of the community must be effected by the causes which have brought about this result.

Another question which emerges is, Whether a special change is observable in the poorer and most overcrowded districts of the city? I have access to information for 1861 and for 1881, and comparing them, I find that while in almost every district of the city improvement has taken place, so that the whole city has unmistakably benefited, distinct improvement has occurred in the Abbey, the Tron, St Giles, the Canongate, and the Grassmarket districts, which you know to be our poorest parts. In them a decrease of mortality varying from 3·77 to 20·71 per thousand has taken place.

The sanitary conditions have been improved during the twenty-five years, in respect of water-supply, drainage, probably improvement of plumber work, and diminution of the density of the population in certain districts. As to the water supply, the total cost of the waterworks up to 1862 had been half a million, by 1886 it had risen to one million sterling. The daily supply of water in 1862 was seven million gallons, in 1886 it was fifteen millions. With regard to drainage, a sum of £260,000 was expended during the quarter of a century, apart from what was spent by proprietors and tenants in connexion with their own property, and of this we may form some estimate from the fact that in the year 1886–7 £13,000 was spent by order of the authorities, and at least £5000 apart from this. Improvements have also taken place as to the domestic sanitary arrangements, both in regard to the introduction of water and the removal of sewage.

The opening up of congested districts by means of the City Improvement Scheme was a most important factor in diminishing the mortality. I remember well Dr William Chambers, when Lord Provost, expressing at a dinner of the Royal College of Physicians the hope that when the scheme, which he was then inaugurating, was carried out the mortality of the poorer districts would become as low as that of the richer parts of the city; and Sir James Simpson, later in the evening, said that if the Lord Provost should succeed in accomplishing so much, he ought to be made an Honorary Fellow of the College. The results attained afford some justification of Dr Chambers's sanguine anticipations. A sum of nearly £560,000 has been expended upon these clearances. Wide and comparatively healthy streets have been opened up through districts formerly infested with disease, and while, for example in the Tron district, the population has been reduced from 314·5 per acre to 178·5, the mortality per 1000 has fallen from 34·5 to 28·9.

Much has also been done by the municipal authorities in the way of providing open spaces as recreation grounds. The establishment of the Arboretum, the purchase of Blackford Hill, the opening of Stockbridge Park and of the Harrison Park, must all prove important aids towards the diminution of mortality, and there are other very weighty factors, such as the diminution of intemperance and the improvement of the quality of food, which must be highly valued.

I am inclined to attach great importance to the system of notification of infectious diseases which Dr Littlejohn has succeeded in introducing among us. You are all familiar with the fact that in accordance with the Police Act of 1879, we are bound to give prompt notice of the occurrence among our patients of cases of the chief zymotic diseases. By the operation of this law the authorities are at once made aware of the occurrence of each individual case of infectious disease as soon as the practitioner in attendance is able to establish a diagnosis. The authorities are thus put in a position which enables them to adopt measures for stamping out the disease before it has time to spread. These measures consist in the isolation of those who have become affected, and, as a general rule, their removal to the Fever Hospital, the removal of neighbours who may have been exposed to the fever poison to houses of refuge, and the disinfecting of the rooms, houses, or tenements in which the outbreak has occurred. By such means many outbreaks of typhus and smallpox have been prevented from becoming formidable, and the severity of other epidemics has been materially mitigated. The authorities have expended upwards of £5000 in fees to medical men for their returns since the system of notification came into action. We may congratulate ourselves, I think, upon the fact that Edinburgh has led the van of improvement in this matter, and I hope that we shall continue to carry out and to extend its beneficent operations. The thought has strongly impressed itself upon some of us that it is absolutely essential that we extend the precautions beyond the urban districts, and that we should get powers to compel notification of a similar kind in the districts from which the milk supply of the city is drawn. The recent formidable outbreak of scarlet fever was, in my opinion, conclusively shown to be due to contamination of the milk supply, and that not from fault of the dairies sending it out, but from their sources of supply in the country. Many valuable lives might have been saved and much formidable illness avoided if the earliest case occurring in that dairy farm had been notified. When the community becomes alive to the importance of this matter, we ought not to have long to wait for satisfactory legislation. I have formulated some suggestions which I think might prove useful as extensions of or additions to our existing arrangements:—

(1.) That the notification of infectious disease should be rendered compulsory in country districts as it now is in the city.

(2.) That power should be given to the authorities to compel the retailers of milk to give complete lists of the sources from which they obtain their milk supply, and also to compel dairy farmers to give lists of retail dairies which they supply.

(3.) That in various centres throughout the counties, hospitals and houses of refuge should be provided, to which the sick and those exposed to infection may be removed, as is at present done in the towns.

(4.) That the city should continue to secure open spaces, especially heights, such as Craiglockhart and Corstorphine Hills, which, though now outside the city, will soon be within its boundary.

(5.) That new buildings and the sanitary arrangements in connexion with them should be very strictly supervised, and perhaps that no property should be allowed to be let unless it be certified to be sanitarily sound.

(6.) That all classes of the community should be encouraged to send cases of zymotic disease to the Fever Hospital for treatment.

(7.) That persons suffering from diseases infectious in their later stages, such as smallpox and scarlet fever, should be rigidly isolated until all danger of their spreading the disease is past.

Original Research.

What are we doing for the advance of medical knowledge? The opportunities for original work have immensely increased in every respect since 1862. The old fields for clinical observation have been vastly extended and new ones have been opened up. The number of medical appointments in the Infirmary and in other institutions has been very greatly increased. We have now, for example, two pathologists constantly at work.

The public museums have been put on a more satisfactory footing, so as to be available for study and research, and the libraries have made great progress, in particular that most valuable of existing collections of medical books in the Royal College of Physicians. Upwards of £10,000 has been expended since 1862 in that institution on the purchase of medical books, and 12,000 volumes have been added to the collection. If it be true, as has been asserted by one of the most distinguished authors of our time, that the bird of scientific discovery lays its eggs at the top of a pyramid of information, access to such a collection is of the utmost moment to all who are engaged in such work.

But new methods have also been introduced, methods by which we can permanently record facts of cases, and preserve the histological features of morbid processes. Above all, we have had an astonishing growth of laboratories in which scientific work may be carried on. Every department of the University Medical Faculty has its laboratory for research. Instead of the dissecting-rooms and the practical chemistry departments being alone provided, as in 1862, we have now every Chair more or less amply supplied. Not only so, but private laboratories have been bringing out good work, and above all these the magnificent Institution just founded by the Royal College of Physicians provides Edinburgh workers with opportunities at least as ample as any to be found in this country. It would be impossible in the time at my disposal to do justice to the original work that Edinburgh has produced during the past twenty-five years, and on the clinical side I shall mention only the share it has had in developing antiseptic surgery and establishing ovariotomy and other operations on a secure and steadfast basis. There can be few among us who have failed to observe with satisfaction the work which has been produced by the laboratory of Materia Medica in connexion with the action of the nitrites, of saline purgatives, and of strophanthus; that of Physiology, especially in relation to the liver functions; that of Surgery, in the whole field of bacteriology; and that of Practice of Physic, in relation to the etiology of phthisis. Nor have the private laboratories been behindhand, for we must all look with pride at the splendid research of Dr Bruce on the anatomy of the nervous system, and at the achievements of our school of obstetricians in relation to pelvic anatomy.

In this and in other medical societies we have also important fields, and I think we can look with no little satisfaction at the use that has been made of the opportunities enjoyed here.

If I may be allowed to offer some suggestions as to the best means of making further advances in this department of our professional duty, I should say—

(1.) That we should take care to keep forward in the line of clinical research, in the accurate and minute investigation of the phenomena of disease and of its treatment.

(2.) That we should seek to employ our improved pathological methods, and the opportunities for laboratory work, by assiduously applying them to the advancement of our clinical knowledge.

(3.) That in order to do ampler justice to the patients, and to make a better use of the material at the command of the School, there should be attached to each physician and each surgeon in the Infirmary an assistant, who should occupy a position like that of the Chefs de Clinique in the Parisian Hospitals. He would, on the one hand, be able in some measure to relieve the medical officers of the burden of responsibility implied in the charge of so many patients, and by his greater leisure would be able to follow out original investigations as to the nature of the maladies beyond what the chiefs and the resident physicians and surgeons are able to overtake. I should not desire to see these gentlemen made permanent members of the staff, but rather that they should hold office for a year or two years, and that the most distinguished of them should be selected for the assistant physicianships and surgeonships when these fall vacant.

(4.) That we should cultivate the habit of co-operation in original work, each collaborateur undertaking the department which his previous knowledge and opportunities have prepared him best to undertake.

(5.) That we should cultivate the habit of attending the societies, and taking a kindly and intelligent interest in the work which is being done by our neighbours.

Education.

The last topic to which I wish to ask your attention is the work which is done by the profession in Edinburgh in the way of training those who are to be our colleagues and successors.

The extraordinary increase of the Medical School in Edinburgh within the past twenty-five years has attracted general attention. In 1861–62 the medical students numbered 543, in 1886–87 they numbered 1872. The teaching has during this period been assuming more and more of a practical character, and the examinations, preliminary and professional, have been steadily increasing in stringency. Some, indeed, maintain that this increase has in some departments been unduly great, certainly the proportion of rejections is very high. The inspectors who visited the University during the final examination in 1885 gave a report which must, on the whole, be considered eminently satisfactory, both as to the character of the examinations and of the teaching; and no doubt the progress made in regard to the licensing of practitioners by the Colleges of Physicians and Surgeons is correspondingly satisfactory. Many causes have doubtless contributed to the success of the Scottish schools of medicine. Among these are no doubt the eminent fame of many of the incumbents of chairs in former days, their achievements having shed a lustre upon the institutions with which they were connected. Much has also been due to the thoroughness and efficiency of the teaching which the Scottish schools have supplied, and associated with this is the peculiar arrangement to which, in my opinion, Edinburgh owes so much, the co-existence and co-operation of university and extra-academical teaching.

During the past quarter of a century many reforms and improvements have been effected in the University system. As to examinations, they have been made in every department more practical than they used to be, and the testing in respect of clinical knowledge has been introduced as an entirely new feature. No student can now receive his qualification without giving proof of personal familiarity with clinical work, and the examinations in the department are so arranged as to test him along the whole line, at least of ordinary medical and surgical work, including gynæcology.

The introduction of co-examiners to act along with the University professors constitutes another most important improvement. Few who have had experience in the matter can doubt the propriety of the professors acting as examiners in their own subjects, while the addition of an independent expert who has no knowledge of the students, or of the special features of the course of teaching which they have had, is equally or almost equally advantageous.

As to the reforms in respect of teaching, I shall not enter into any detail, excepting in so far as it concerns the teaching of medicine, with which I am specially connected. The growth of the School has made it essential that our cliniques should be subdivided, and accordingly, instead of, as was formerly the case, one professor being on duty at a time, and being solely responsible during three months of the session for the teaching of the clinical class, all the clinical professors now remain on duty throughout the whole year, the class thereby being subdivided into manageable groups. The Professor of Midwifery has also become associated with other colleagues in clinical teaching, and supplies the practical training in gynæcology, for which few opportunities were afforded formerly, and which are acknowledged to be of the utmost importance. The introduction of tutorial drill in the various technical methods of examination of patients has constituted another valuable step in advance. These tutorial classes have been organized with extraordinary skill, and carried out with remarkable efficiency, and have materially helped the students in their efforts to acquire a knowledge of medicine.

The establishment of special cliniques in the Infirmary upon syphilitic diseases, skin diseases, diseases of the ear and throat, and the much fuller development of the department devoted to diseases of the eye, also claim a place in the list of important reforms. The opening up of the Royal Hospital for Sick Children, by the establishment of clinical lectureships in connexion with the University and with the extra-academical courses, marks very important progress; and I do not doubt that in a few years further developments will have taken place in connexion with that invaluable institution. The clinical surgery of children's diseases is nearly as important as their clinical medicine, and both that and the subjects hitherto specially taught will come to occupy important positions in the curriculum.

The clinical teaching of fever, which is so essential, has within the past few months been placed upon a footing more satisfactory than it has been since the City Hospital was opened; and notwithstanding the dangers and disadvantages which attend the clinical study of fevers, I am confident that this Society will agree with me in thinking that no student can be regarded as properly trained, or fitted to enter upon practice, who has not had opportunity of studying that important class of diseases at the bedside. I wish that it had been possible to persuade the authorities to appoint two clinical physicians, who should also have been clinical lecturers on fever; but the arrangements which they deemed it wiser to make are certainly being turned to the advantage of the students as far as possible. Reforms have also been effected in regard to the teaching of insanity. The severance of that specialty from its connexion with the Chair of the Practice of Medicine was, in my opinion, a step in the right direction; and there is no school where those who wish to make themselves masters of this department have better opportunity of doing so than in Edinburgh.

I have heard the idea expressed that medical teaching in Edinburgh affords too much of mere instruction and too little of real education; that the student is not left sufficiently to find things out for himself, but is trained to depend entirely upon having them put before him in the very simplest forms. This question deserves attention; for if it be really true, it implies a very serious defect in the educational system. But is it an objectionable thing that the facts, principles, and theories which a student of medicine must master be presented to him in a readily intelligible form? Is that not preferable to his having them presented in a crude, obscure form, or in one difficult of comprehension? So long as care is taken in the examinations that mere answers from memory shall not suffice for the purpose in view, but that questions shall be framed so as to elicit evidence of a really intelligent acquaintance with the subject, we may be sure that no evil will follow its simple and vivid presentation to the student's mind. Is it not, on the other hand, true that students now are taught to do things with their own hands, and to think out questions for themselves, in a way that was not dreamt of five-and-twenty years ago; that they are initiated into practical work, and have the opportunity of developing their powers as fully in the educational institutions of which I am speaking as is the case in any of the schools in any department of human knowledge?

As to suggestions by way of further improvement, I shall venture to urge upon this Society—

(1.) The importance of our presenting a united front, and not allowing local prejudices or local jealousies to ally themselves with the very active opposition which is more or less constantly threatening the School.

(2.) The necessity of rearranging the examinations, or of extending the curriculum so that students may be able to devote sufficient time to the study of the practical subjects; too much of the four years at present required is devoted to departments which, although no doubt of great interest and value, are not to be the lifework of medical practitioners, and of which a knowledge is not of vital moment, as a knowledge of Medicine, Surgery, and Midwifery undoubtedly is.

(3.) The hospital hours should be so altered as to extend over at least three instead of two hours daily as at present, so that students might be able to avail themselves of the opportunities of clinical study in the medical and surgical wards simultaneously, if that seemed desirable, or to attend senior cliniques in the surgical and special cliniques in either department, along with the ordinary medical cliniques during the later years of their studies.

(4.) That attendance should be made compulsory upon cliniques on children's diseases and on fevers—say, courses of twenty to thirty cliniques upon each—and that special short clinical courses—say of twelve demonstrations—on mental diseases should be made compulsory for every student, while encouragement should be given to those who wish to make insanity a specialty to attend the more extended course as at present conducted.

(5.) Another question of much interest is whether attempts should not be made to establish residential colleges, inns, halls, or boarding-houses for the benefit of such students as might incline to take advantage of them in preference to the present system of boarding in families or living in lodgings. I should regret exceedingly if it were attempted to make such a plan universal; but I believe that important ends would be served if such institutions were founded in connexion with the Medical Schools of Scotland.

I have to thank you, gentlemen, for the patient hearing of these desultory observations, and for all the courtesy that I have experienced at your hands during the years that I have had the honour of occupying the presidential chair. When we remember that our President elect has already, with eminent success, occupied the Chair of the Royal College of Surgeons, and is universally known among us as one of the most genial and cultured, as well as able of the members of the profession, we have good reason to hope that the Society may go on to greater prosperity during his tenure of office. Too often in the history of the Society have I remarked that the termination of a Presidentship coincided with the disappearance of the President from all future meetings of the Society. I hope that it will not be so with me, for I fully intend to be as regular in the future as during the past two years. I have never yet attended a meeting of this Society without learning something that I was glad to know. I wish that we could see the Society characterized by the attendance of the senior members of the profession as well as by the juniors, by the communication of good papers, embodying the results of careful and able work, and by their discussion in short and pointed, appreciative and yet critical speeches.

V. Original Communication.

ARTIFICIAL LEGS.

Part I.—Should partial Foot Amputations be abandoned?

By Charles W. Cathcart, M.B., F.R.C.S. Eng. and Edin., Assistant-Surgeon, Royal Infirmary, Edinburgh; Lecturer on Surgery, Edinburgh School of Medicine.

In the progress of the art of Surgery, as of other arts, it becomes from time to time necessary to readjust rules and maxims so that they may keep pace with growing knowledge. Rules of procedure, which may be the best possible in certain states of surgical attainment, or with certain conditions of allied arts and sciences, become obsolete, useless, and even sometimes harmful, as the correct guides to treatment when the conditions on which they were based have changed. We are all familiar, for example, with the changes in surgical practice which antiseptic treatment has introduced, especially where healthy bones and joints require to be dealt with,—so much so, in fact, that many operations which formerly were considered unjustifiable are now actually called for. This is sufficiently evident, and we need not dwell on it, but we may reasonably apply the general principle to our present subject, and see how far old rules for the sites of amputation should be allowed to guide us still.

In a recent work,[1] an eminent artificial limb maker has formulated a request that the old rules should be reconsidered, and has urged two apparently reasonable grounds for doing so,—*i.e.*, that risks after amputation are much less than they were, while the skill and resources of artificial limb makers are much greater. But what are the old rules for the site of amputation in the lower limbs? Briefly they are summed up in the phrase, "the least sacrifice of parts," except in section of the tibia, where, for poor patients at least, the aim is to leave not more than about 2½ inches below the knee. The reason for removing as little as possible was simply that it had been shown by statistics, and generally accepted, that the nearer the amputation was to the trunk the greater the risk to the patient, so that "least sacrifice of parts" was synonymous with "least risk to the patient." In examining the causes of death after amputations thirty or forty years ago, we find that a large share was taken by pyæmia, septicæmia, and other complications of wounds, which have been greatly if not quite removed by antiseptic precautions. Hence, if a new series of statistics were made, embodying the results of recent improvements, it is *possible* that within certain limits it might no longer be true that the "least sacrifice of parts" was equivalent to "least risk." If this were established, and if, also, it were definitely proved, on the other hand, that the surgeon should at many points amputate more freely than formerly in order to allow the best possible artificial substitute to be afterwards fitted on, we should indeed have reason to reconsider our rules as to the best sites for amputation. We see, therefore, that there are two main questions to be considered:—(1.) Is it desirable to modify the old rule of least sacrifice of parts? This naturally comes first, for only if it be answered in the affirmative comes the next. (2.) Is it justifiable in view of the risk to the patient?

Let us begin with amputations of the foot, and to avoid misunderstanding let me say at once that I do not intend to discuss in *what cases* partial foot amputations are indicated, but whether or not they are ever indicated at all.

[1] *Artificial Limbs*, by Heather Bigg, Assoc. Inst. C.E., etc., 1888.

The question of desirability, as already stated, has been definitely raised in recent times by instrument makers both in Great Britain and in America. Thus Heather Bigg, previously referred to, says, "The rules may be epitomized thus: Save the tread of the foot; if this is impossible, save the heel, and do Busk's modification of Pirogoff. If the heel cannot be saved, save the knee-joint and its due length of stump, and amputate somewhere in the middle third of the lower leg (tibia); and if the knee and its functions cannot thus be preserved, amputate as low in the middle third of the thigh (femur) as possible" (page 20). As a corollary to this we find on page 22,—"If all the toes are removed, as sometimes is done in cases of frost-bite or crushing injuries, the tread of the foot is lost, and experience has shown me that it is better, therefore, to remove the whole foot. And if this is the case, how much more, *a fortiori*, is it better to remove the entire foot than to perform any such operations as Hey's, Lisfranc's, or Chopart's? For in all of these the heel is left, but in front of the heel remains a portion of the foot of greater or less length, which is absolutely useless!" etc.

Again, in the pamphlet descriptive of the well-known American "Bly Leg," the writer gives under the head of "Points of Election," page 36, the following summary,—"Therefore the junction of the middle or lower third of the tibia should be the *first point of election* (*sic*) whenever the flexors of the foot cannot be saved." In reading the explanation previous to the above, one sees that flexors of the *ankle* are indicated rather than of the foot proper. It is there stated that "the operation known as 'Chopart's' severs the flexors of the foot, and should never be performed under any circumstances whatever. The moment the flexors are severed, the extensors, having no antagonists, draw the heel upward, extend the foot on the leg, and cause the amputated surface to point almost directly downward." The same words are used also by the author of the pamphlet descriptive of the "Foster Limbs."

If it were necessary we might add to these, but it is sufficient for the present that these three artificial limb makers of large experience consider that "least sacrifice of parts" is by no means always the best indication for giving a patient the most suitable stump for the fitting of an artificial limb. Let us, therefore, try to discuss it fairly in all its bearings. In attempting to answer the question, "How far is 'the least sacrifice of parts' desirable?" we have two lines of inquiry before us,—(1) the physiological or theoretical, (2) the empirical or practical. The first will clear up and simplify the second, and if correctly followed out, should lead to the same results. The second, however, when its facts are fairly interpreted, will be our final court of appeal. In following it out, we shall consider the experience of some surgeons and of as many instrument and artificial limb makers as we have been able to reach.

1. The physiological line of inquiry is simply an investigation into the part taken in the mechanism of walking by the various joints of the lower limb. Much of the knowledge accumulated by successive workers in the field is of little service for our present purpose. It is unnecessary to review here what has been done by the brothers Weber, Professors Marey, Carlet, and others; but of all the methods employed, two seem to be best suited to give us the information we want. These two are, ordinary careful observation, alone, and aided by instantaneous photography.

The methods of applying photography for the analysis of movements have been brought to high perfection by Mr Muybridge in America and by Professor Marey in France. Unfortunately, however, the results of their work on the step in man have not been published in any easily accessible form, and are not—like the views of the galloping or trotting horse—available for general reference. On this account, and being much interested in the subject, I had some years ago several instantaneous photographs taken of persons walking. The subjects were students, members of the University Athletic Club, who were sufficiently devoted to science to allow themselves to be photographed walking without their trousers. Several of the attempts failed, but the following four successful ones are available for reference. One is a view of four athletes walking across the field of the camera, all out of step, so that each represents a different phase of action (Figs. 1, 4, 5, and 6). Another is a side view from a nearer point of a single athlete. His action has been caught just as his left toe is leaving the ground (Fig. 3). The same one was taken from the front in another view; and the last is a side view, in ordinary costume, of the student Mr Wyllie, who had kindly taken all the other photographs for me (Fig. 2). These, it may be said, are very few to go by, but after a careful study of them, and after having compared them with a series by Marey (published in the February supplement, 1887, of the *Scientific American*), with numerous examples of persons walking which appear in instantaneous photographs of crowded streets, and with what one can observe upon one's own walk and on that of others, I have seen no reason to doubt their value as correct representations of the normal step in walking. The following account of the step, therefore, has been worked out partly from these sources of observation, and partly from the written accounts by the Webers, Carlet, Ward, and especially Marey. I have to acknowledge with gratitude Professor Marey's great kindness in sending me reprints of his valuable papers which from time to time have appeared in the *Comptes Rendus*.

It will help to keep our ideas clear if we first examine the ends which seem to be attained in walking. They are the carrying of the body forward at as nearly as possible a uniform rate, its maintenance at as nearly as possible the same level, and at as great a saving of labour as possible.

Uniformity in the rate of forward progression is attained by the propelling action of one leg being kept up until the other is ready to take its place. The resistant shock of the leg extended forward is met by the obliquity and hence greater propelling power of the other leg then extended obliquely backwards. The shock is also lessened by the giving way of the advanced knee as soon as pressure begins to bear upon it.

Uniformity of level in the carriage of the trunk is effected as follows:—The limbs when in their straightened and hence extended state are always more or less oblique in position; when vertically below the body they are more or less flexed. This explains what the Webers long ago pointed out, that a man in walking carries his head and trunk lower than he does when standing, because it is only in the latter position that his legs are straightened vertically below him. As every one is aware, however, there is a certain rise and fall of the trunk at every step. The rise occurs almost simultaneously with or slightly after the trunk has passed over the supporting leg, when the knee, previously slightly bent, is extended again, the heel being still on the ground (Fig. 4). Although this does not seem to agree with M. Marey's diagram, Fig. 7, it corresponds with his description. When speaking of the curve described by the hip while the foot is on the ground, he says, "Le maximum de hauteur de cette courbe au-dessus du plan horizontal ne correspond pas au moment où l'articulation de la hanche passe en Y, verticalement au-dessus de la base de sustentation formée par le pied, mais se projette un peu en avant de cette base, dans le sens de la progression."[1] The degree of forward obliquity of the supporting leg while the knee is being straightened has the effect of propelling the body forward as well as of raising it. It is of interest to note that the rise occurs as the non-supporting limb is sweeping past the body to get into position for the next step (Fig. 4). The fall occurs from this point until the advancing heel touches the ground (Fig. 6, and E, Fig. 7). Since the trajectory of the knee is nearly in a straight line ($G\ G_1$, Fig. 7), this fall corresponds to an increasing obliquity of the thigh. (The heel seems to leave the ground at different stages of the step according to the rate of walking. When the pace is moderate, the rise by extension at the ankle does not occur until the advanced heel has touched the ground. In fast walking, where also the stride is longer and the legs more oblique, this rise occurs earlier.)

Saving of Labour.—This is attained by uniformity of rate and of level (so far as it goes), and is further aided by the bending of the joints of the limb which is being carried past the other. The shortening thus produced permits of the limb being swung forward with a much less exertion of muscular energy than would have been necessary had the limb remained straight. In that

[1] Marey. *Comptes rendus*, May 19, 1884.


FIG. 1.

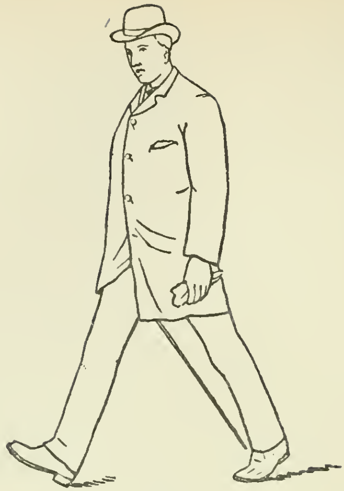


FIG. 2.

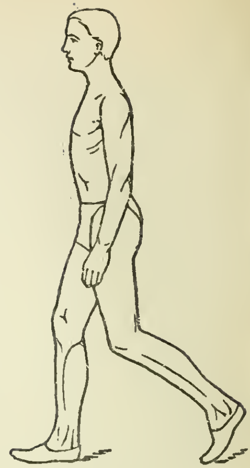


FIG. 3.


FIG. 4.


FIG. 5.


FIG. 6.

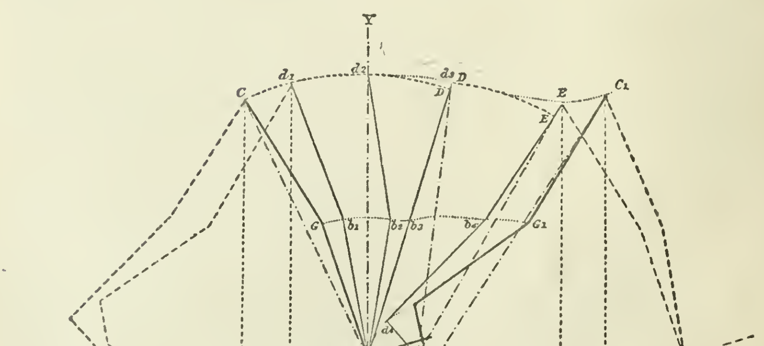


FIG. 7.

case, as in the straight pin leg, the leg must have been abducted as it was carried forwards.

If we look now at the part played by the two limbs in walking we can best analyze the movements by dividing them into two stages—(1), The active or propelling stage; and (2), the passive stage, where the limb is merely being carried into position for the next step. The first is the one on which the advance of the body really depends, although without the second the movement could not be repeated. The speed in the second stage is much greater than that in the first, as in the second the limb must swing from behind forwards and rest on the ground in front till the body comes over it, in the time that the other limb, resting on the ground, has passed from a slightly oblique backward to an oblique forward position (Fig. 3, interval between two steps).

(1.) During the active stage the propelling power is gained by the action of the following joints. As one leg is just finishing its propulsion, and as the body is coming over the supporting leg, its knee-joint is slightly bent (Figs. 3 and 7, d_2 b_2), but as soon as the trunk has passed over this leg, which becomes then relatively oblique, the knee is straightened, thus raising the body and propelling it forward (Figs. 4 and 7, D A). The further advance of the body is kept up by the extension of the thigh, accompanied first by flexion of the ankle and afterwards by extension of this joint as the heel is raised from the ground. The play of the ankle is thus of use not merely in the raising of the heel, but in the oblique movements of the leg, steadying it while the sole is planted on the ground. "L'Angle que la jambe forme avec le pied change aux différentes phases de l'appui: pendant la première phase, celle de l'appui du talon et de la plante; la jambe se fléchit graduellement sur le pied; pendant la seconde, à partir du moment où le talon se soulève, le pied s'étend graduellement sur la jambe jusqu'à l'instant où il se détache du sol."[1] The lateral movements by the foot, too, controlled as they are by muscles, will greatly facilitate steady balancing of the body while it is being supported and carried forward. Besides the extension of the thigh, there is also a movement of rotation; but as the natural hip-joint is available for most artificial limbs, we need not discuss it further.

(2.) During the passive stage of the movement, as the toes leave the ground, the thigh and ankle joints are extended, the knee flexed, and the toes bent towards the dorsum of the foot. As the leg swings forward the thigh and ankle joints flex, while the knee remaining flexed at first extends again as the heel comes to the ground. As soon as the heel has touched the ground the ankle extends so as to plant the sole flat. Meanwhile, the advanced leg rapidly flexes over the planted foot again, while the

[1] Marey, *loc. cit.*

knee yields slightly as the body passes over it, the thigh passing gradually from flexion into extension.

We are now in a position to consider what may and what may not be expected of artificial joints. It will be at once evident that they will fail most in the active propelling stage, where they can only act passively, *i.e.*, yielding to movements impressed upon them by parts on which muscles act. Hence, though they may appear to act in a natural way, they cannot contribute to uniformity of rate of progression, and must be acted on by the active joints which remain. In the passive stage, however, we can expect a much closer resemblance to the natural movements of knee and ankle by the action of springs which have been compressed or stretched during the active stage, either by the weight of the body or by the action of parts of the limb which remain.

Hence we may draw this general conclusion, that the greater the muscular power which a patient has over his stump, and the more of his own joints which he can actively control, the less damage will there be to his walking power during the propelling stage of each step, and the less fatigue is he likely to suffer. Further, that even where a stump is too short to actively control the artificial limb to be fixed to it, the muscles fixed to this short stump, which act on the joint above, will have a leverage on this upper joint which must mostly be lost if their lower attachments are severed.

If, now, from the physiological point of view, we were to suggest rules for the site of amputation, we would say—If you cannot leave a patient the tread of his foot so that he can raise his heel, leave him the lateral movements at the inter-tarsal joints, and the play of his ankle, so that he can balance himself well while his stump is on the ground. If you cannot leave the lateral movements, leave his flexion and extension of the ankle, which will serve him well in accommodation while his knee and thigh joints are propelling. If you must sacrifice his ankle, leave his knee-joint and as much below as you can for leverage. If his knee must go, let him have as long a thigh stump as possible, for it now must do all the work; in other words, *physiology leads us to "the least sacrifice of parts."*

But the question of "bearing" might now be raised; and on the above lines it seems evident that the lower the bearing *can* be taken the more effect will it give to joints above it; but that where bearing cannot be taken with comfort below a joint, a long stump will still be of service, since by side pressure it can more effectively force the artificial limb into any desired position. It would therefore seem (1), that it is a mechanical disadvantage to take the bearing of artificial limbs at the ischium if, with comfort to the patient, lower points can be found to tolerate it; (2), that the absence of bearing points below a joint is no reason for neglecting the "least sacrifice of parts."

2. Passing next to consider the empirical line of inquiry, we may take first the opinion of various instrument and artificial limb makers. In order to see how far the opinions held by some members of these professions were shared by others, I drew up a few questions, which, with an explanatory letter, I sent to as many as I could find. In all eighteen letters were sent out (including three to American firms), and to these I have received twelve replies. I wish to acknowledge here my sincere thanks to those firms who have so kindly complied with my request for information.

In order to concentrate attention, we may take first partial amputation of the foot. In referring to the answers, I have distinguished between makers of and dealers in artificial limbs, whom we may term M and D respectively. The first four answers come from dealers, the remainder from makers. The first question was—

"How in your experience do the stumps left after a Hey's (or Lisfranc's), or a Chopart's amputation, compare with those left after a Pirogoff's or Syme's amputation as regards—(1), Usefulness; (2), Appearance of the Artificial Foot?"

The only two answers favourable to partial amputations are the following,—No. 3 says, "Have had very few Chopart's, but succeeded very well. We have never any difficulty with a Syme's operation." The other, No. 8, writes, "I prefer the operations you refer to in the following order—1, Chopart's; 2, Pirogoff's; 3, Syme's; 4, Lisfranc's." The effect of this, however, is neutralized by a subsequent note, "My preference for Chopart's operation is not so much over Pirogoff's or Syme's as over Lisfranc's." All the rest prefer either a Syme or a Pirogoff to any partial operation.

We may take it, then, that the majority of artificial limb makers think badly of the value of a Chopart's or Hey's amputation.

Many surgeons, again, share this view, but still there are a good number who do not, and to some of these I may refer. Thus Mr Erichsen in his last edition says, "The result of this operation (Chopart's) is extremely favourable, the patient by the aid of a properly constructed boot being able to walk and even dance with very little appearance of lameness" (Vol. ii. p. 113).

The late Mr Syme also seems by no means to have intended his special operation to replace Chopart's amputation for cases suitable to it. In his *Contribution to the Pathology and Practice of Surgery*, published in 1848, he says—

"The operation of Chopart might frequently have accomplished all that was requisite, but unfortunately suffered from a prejudice which opposed its adoption. This was, that the extensors of the heel, being deprived of antagonizing action, would point the stump downwards, so as to render it useless as a support for the body. In 1829, for reasons elsewhere stated (*Edin. Med. and Surgical Journal*, 1829), though there was no precedent for its performance in Edinburgh, I ventured on this partial amputation of the foot with perfect success, and without the slightest inconvenience of the kind anticipated. Encouraged by this result, I resolved to adopt the operation, and before long performed it six times with entire satisfaction. Since that time the operation has been established here, and regularly practised in cases admitting of its application."

Mr Miller has kindly directed my attention to a paper by Dr J. B. Murdoch of Pittsburg, U.S., in the *Medical News* for July 16, 1887, where the details of a case of double Chopart are related. The patient was a mulatto. He had lain out on an intensely cold night while drunk, and the fore part of each foot had become gangrenous from frost-bite. Dr Murdoch performed a Chopart on each foot, dividing the tendo Achilles at the same time. Leather splints were moulded to his legs and to the remains of the sole of his foot to keep the heel extended. At the end of four weeks he was going about on crutches, and left the hospital sixty-three days from the date of admission. He was not seen again for three and a half years, when the result was found very satisfactory. "After his dismissal from the hospital the patient informed me he continued to wear the splints, which had been moulded to his legs at the time of the operation, for three or four months, when he discarded them, and has since worn nothing but a pair of ordinary leather gaiters laced in front. He keeps the front of his shoes stuffed with cotton or other soft material. He is engaged as a cook in a boarding-house; he is able to walk to his home, a distance of four long city blocks; he frequently walks down town to the post-office and back, a distance of two miles, with no other assistance than an ordinary cane. He is able to flex and extend both ankle-joints with perfect ease and to their full extent. He asserts that his feet give him no pain, and that he would much prefer to have them as they are than to have them off at the ankle." . . . Further on he says, "I could state other cases of Chopart's amputation, which have come under my observation, where the result was much better than it is possible for any of Syme's amputations to be." One of these he narrates; the result seemed to be excellent. The nature of the appliance worn is not explained.

Other instances might easily be given, but these are sufficient to illustrate the side favourable to Chopart's, *i.e.*, one of the partial foot amputations. As it seems to me, Hey's, or some of its modifications, is even more favourable than Chopart's.

I refrain from quoting opinions of surgeons adverse to partial foot operations, because the objections are so well known as hardly to require confirmation.

So far as my own observation is concerned, it leads me to approve of partial foot amputation. Since I began this inquiry, I have only been able to trace out two patients on whom such operations have been performed. (1.) A railway porter on whose left foot a Chopart's amputation was performed by Dr J. Shaw

M'Laren, when house-surgeon in Chalmers's Hospital, in 1886. The man bears his whole weight on what remains of the sole of his foot, and walks with ease and comfort. He cannot, of course, rise on the toe of his boot by extension at the ankle, and so takes a shorter step with the left leg; but as he gets his weight forward over the partly amputated foot, his heel is raised from the ground. With the exception of a slight halt his gait is natural. He can flex and extend what remains of his foot, and feels it a great help to him in walking. He would not part with it for anything. In the sole of his boot he wears a steel bent up so as to raise the fore part of the stump; the front of his boot is stuffed and padded. Owing to the sinking of the arch the shape of the boot is clumsy, but were appearance any special object to the patient this might be improved. The muscular development of this leg compared with the other is markedly less (Fig. 8). (2.) A young man, æt. 24, on whom, for a railway smash, Dr John Duncan, in January 1884, performed a Syme's amputation on the left side, and a Lisfranc's on the right. Both stumps healed well, and the case seems a peculiarly suitable one for comparing the two operations. The artificial foot fitted on to the Syme's stump is much neater, and *seems* to act more naturally than the somewhat distorted boot enclosing the remains of the other foot. On questioning the patient, however, I found that the latter was of much greater use to him, and in order to have his own opinion on black and white, I wrote to ask him some questions, to which the following is his reply:—" *4th October* 1887. —Sir, I am in receipt of your favour, and beg to say, in answer to your first question, that my occupation prevents me from telling how many miles I am able to walk without any fatigue, but on Sunday afternoons (this being the only day I am at liberty) I generally go a distance of four or five miles constant walking without the least fatigue, and calculate that I can walk three miles in the hour easily. (2nd.) I have the greatest comfort in the stump with the partial removal of the foot, and most security with it in walking. (3rd.) I am heartily thankful that only part of the foot was cut off, for had it been removed at the ankle like the other, I am quite confident I could not walk near so well—the half foot assisting very much to keep me balanced, and more confidence in walking.—Yours, etc."

In the Lisfranc's stump he has lateral motion as well as full flexion and extension of the ankle, and as the apparatus he wears gives scope to these movements, it is easy to see how the balancing power which this gives him is felt to be such an advantage over the Syme's stump. The muscles of the leg show a development corresponding to the loss of parts. The calf muscles above the Syme's stump are nearly entirely wasted, and he feels the leg generally cold. On the other side (Lisfranc's operation) the calf muscles are larger, and the leg is more easily kept warm (Fig. 9).

Fig. 8.

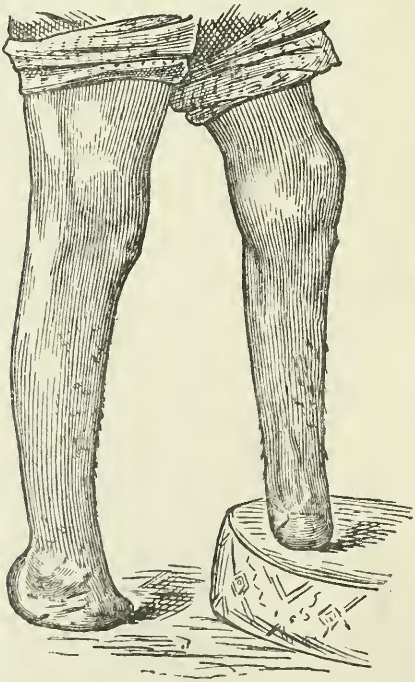


Fig. 9.

Our examination of this case, coupled with what we have hitherto brought forward, will perhaps help us to explain discrepancies of opinion with regard to the partial foot operations. If *appearance* is the first requisite, an entire artificial foot fitted to a Syme or Pirogoff's stump will be indicated rather than any partial operation. If *usefulness* is most of all wanted, the preference will be reversed.

But this does not explain everything. Many distinctly object to partial foot operations on the ground of their giving bad results. How are their arguments to be met? Some of these objectors use a physiological argument, and say with Heather Bigg—"If the perfect base, with its tread, cannot be saved, then the ankle-joint is of no further use, and may go too."[1] This, as we may see, from our previous studies, is based on an imperfect understanding of the action of the joints of the foot and ankle in walking. It is, moreover, pernicious if used as the theory on which an apparatus is to be made for partial foot operations. Others make use of the old and oft-repeated argument, that if the tread of the foot be gone, and the tendons on the dorsum of the foot be divided, the tendo Achilles *must* draw up the heel and point the scar on to the ground so as to make walking impossible. Theoretically this sounds well; practically it is not true. It may, of course, have been true in some cases, just as there have been unfortunate results with any and all stumps, but it is probably much less liable to occur now than formerly, when wound treatment was prolonged and suppuration extensive. In Dr M'Laren's case the tendo Achilles was not divided, and no special care taken to bandage the stump in position. The leg was flexed and laid on its outer side to relieve the gastrocnemius. As the result has shown, this latter precaution was sufficient. Other means are very easily taken, however. Dr Murdoch used them, though he also might perhaps have done as well without them. After the wound has healed and the patient begins to walk, we need have no fear. The divided ankle flexors take up new attachments to the cicatrix. Muscular development also accommodates itself so well to its requirements that the posterior calf muscles will very soon be in a state to do just the amount of work required of them and no more, and they will have no superfluous energy left for pulling up the heel and forcing the cicatrix on to the ground. This is so general a law that it is hardly necessary to do more than point out its exemplification in the cases before us. When in the first case the leg muscles for acting on the Chopart are compared with those belonging to the sound foot a manifest deficiency is seen, but not more than is apparent in the second case, when the muscles belonging to the Syme's amputation are compared with those of the other leg (Lisfranc's operation). Thus, even in a Chopart, and without the usual precautions to the contrary, the drawing up of the heel does not necessarily occur.

[1] *Op. cit.*, p. 35.

And if any tendency should exist at first, when antagonized it will disappear.

Lastly, as to the apparatus for these partial foot amputations. On physiological grounds we would expect that apparatus to succeed best which permitted freest use of the natural joints which remain, and so we find it. In all the cases which I have seen, and most that I have heard of, where a Chopart's or Lisfranc's amputation was a success, the simplest apparatus has been used—a steel in the sole of an ordinary boot, with stuffing in the toes; or, simple stuffing or cork in the front of an ordinary boot where the lost parts should be. Sometimes a leg gaiter is used to keep the boot on; sometimes the lacing of the boot round the ankle has been enough. On the other hand, if Mr Bigg may be taken as an exponent of those who object to partial foot amputations, the somewhat complicated nature of the apparatus which he describes, ignoring as it does the natural ankle-joint movements and putting in an artificial one, seems to me to be the main cause of its frequent failure. The same objection would hold against the Government appliance which he also describes. And not only is there an objection to this kind of apparatus for a Chopart (described as the only possible one under the circumstances by Mr Bigg), but there is a similar objection to every kind of apparatus where the same plan is adopted. When two portions of an artificial limb are jointed together near a stump face, the bearing should always be taken on the upper portion of the two or above it, if friction on the end of the stump is to be avoided. When this is done, the stump and the part on which it rests always move together, whereas if the bearing is taken on the part below the axis of movement, friction as well as pressure on the end of the stump is inevitable. In the one case it is the artificial joint which has to transmit the weight as well as stand the friction, and in the other case it is the face of the stump which has both to bear. The principle can be carried out in one or other of two ways—(1.) By fixing the axis of movement below the upper piece on which there is the bearing; (2.) By pivoting the upper bearing part on rule joints above its lower end should the first plan involve too low a position for the axis of movement. The necessity for rule joints bearing weight may be a disadvantage, but seldom a greater one than is involved by the friction on the end of the stump. In the common apparatus for a Syme's amputation, the axis of motion is further from the ground than that of the natural ankle-joint. The faults I have indicated are common in the apparatus for a Syme's amputation. Many stumps can, it is true, stand the friction, but others are rendered so sensitive by it that walking with the artificial foot becomes impossible, although the stump is able to bear the weight of the body by steady direct pressure. Recently, in a case of this kind, after the above suggestion had been carried out, a patient was able to walk perfectly, although previously she had been unable to use her artificial foot without great pain. Since this principle has not been carried out in many of the so-called failures of *partial* foot amputations, we have a satisfactory explanation of why the failure may have occurred.

It will be noticed that I do not attempt to answer the second question with which I set out, *i.e.*, "Is it *justifiable* to modify the rule of least sacrifice of parts in foot amputations?" because the first question, "Is it on other grounds *desirable?*" has been answered in the negative.

To recapitulate briefly what we have seen, the reasons for this negative answer are—that while the most careful physiological investigation points clearly in favour of partial foot amputations, practical experience of the results very fully bears this preference out; and that although many instrument makers and some surgeons are opposed to these operations, a satisfactory explanation for their opposition may be found—(1), In the occasional failure of these as of other amputations; (2), In the erroneous views of the mechanism of the natural joints in walking; (3), In the prevalence of faulty apparatus; and (4), in the undoubtedly superior *appearance* of the artificial feet fitted on to amputations of the entire foot.

Mr Miller said the paper had made an impression on him which would probably appear in his future operations. He had no doubt that Mr Cathcart agreed with him, that what he had said did not hold in regard to cases of disease, because in them Mr Syme's adage would come in, that the removal of the whole foot was necessary to prevent the recurrence of disease in the tarsus. He had no doubt that in many cases in which Syme's operation was done for injury, the surgeon was influenced in his election of site by the fear of gangrene or sloughing of flaps, which might occur in the case of a Chopart or Hey. In his ward he had two cases, showing the advantage of not operating at once in cases of injury to the foot. The first was a boy whose toes were gangrenous before his admission. Had he operated immediately after the injury, he should very probably have done a Syme on the one foot and a Hey on the other. The toes were, however, left to separate, and the result was two useful feet. The other case was that of a man who had his foot crushed by a piece of coal falling on it in a mine. His toes were gangrenous when he came in some days after the injury. Had he amputated he should have done a Syme, but he did not. The man now had a useful foot minus his toes. All Syme stumps were not so apparently useless as the one shown by Mr Cathcart that evening. He knew cases with very great mobility of the stump. In these cases the operation had been done in childhood, and this might be the explanation of the want of mobility which had been demonstrated, the operation being done in adult life. With these reservations he admitted the great merit of Mr Cathcart's paper.

Prof. Chiene was glad Mr Miller had laid stress on the mobility of the Syme stump. He hoped Mr Cathcart would continue to work at this subject, as the science of artificial limbs had passed too much out of the surgeon's hands into the hands of instrument makers.

Mr Cathcart, in reply, said he meant to exclude the question of disease. The question was whether or not Chopart's amputation should be done under any circumstances, not in what cases might it be done. Heather Bigg would sweep it away altogether. He (Mr Cathcart) had tried to show that the operation was in itself good, provided the case were suitable.

Special Meeting.—November 16, 1887.

Dr John Smith, *President, in the Chair.*

I. Election of Ordinary Members.

The following gentlemen were elected ordinary members:—John C. Messer, M.D., R.N., and David Wallace, M.B., C.M.

II. Discussion on the Etiology of Tumours.

ABSTRACT OF REMARKS OPENING THE DISCUSSION.

By G. Sims Woodhead, M.D., F.R.C.P.Ed., Superintendent of the Royal College of Physicians' Laboratory, etc.

Mr President and Gentlemen,—When I accepted the invitation of your Secretary, Mr Cathcart, to open the discussion on this subject, I was, to a certain extent, prepared, as some three years ago I attempted to put into shape my ideas on the subject of the etiology of malignant tumours. In the interval some of my views have gone by the board, others have been modified, but as to the main opinions, time and observation of facts have merely strengthened them. Unfortunately for us, our real information on the subject of tumours is as yet comparatively limited. We know something of their clinical history, of their naked eye and microscopic appearances, but beyond these points our knowledge is of the vaguest and most unsatisfactory character. To-night, Mr President and Gentlemen, should I wander into the region of speculation and theory, I must crave your indulgence, and must at the same time ask you to remember that up to the present all has been theory.

Cohnheim, whose belated rudiment theory we have all so thoroughly admired, was never able to demonstrate his islets of permanent embryonic tissue; and it must be confessed, by even Cohnheim's most ardent admirers, amongst whom I am proud to class myself, that his theory of ensnared and persistent embryonic rudiments and the relation of these to malignant tumours, especially of old people, will not conform to all, or even a majority of all the facts already at our disposal. That certain tumours, especially certain simple and certain cystic tumours, and even some malignant tumours, as in the case of extremely malignant growths in children, may be accounted for on the assumption that a modification of Cohnheim's theory is correct, I am quite ready to admit, but beyond this I cannot go. I am further willing to admit that there is a possibility, nay, even a probability, that in many cases it may be proved that some of the growths described as malignant tumours may be relegated to the class of infective granulomata, especially in the case of certain rapidly spreading and rapidly degenerating sarcomata, and also in the case of certain lymphadenoid growths. Lastly, although I cannot believe in these tumours as the result of blood disease, my firm conviction is that many of them are the result of altered constitutional conditions, including in the term constitution the vital conditions of the various tissues which make up the organism.

Before we can make much progress with our subject, we must go back to first principles, for in this subject, as in all others with which the clinician and pathologist have to deal, we must rely in great measure for our knowledge of the abnormal on a thorough study of the normal. And in our study of the development of embryonic tissues in the adult, we must avail ourselves of the knowledge which has been accumulated on the development of these tissues in the embryo. I propose, therefore, to-night to include what I have to say under the following headings:—(1), What we may learn from certain points to be observed during the early stages of development of various forms of ova; (2), knowledge to be derived from the study of normal cells on the lines laid down by Goodsir and Virchow and more recent biologists; (3), the application of the laws of nutrition and heredity formulated by the Spencerian school; (4), some of Sir James Paget's statistics as to tumours; (5), my own observations bearing on some of these points. I shall not attempt to classify my remarks under these headings, but most of what I have to say will come under one or other of them.

Segmentation of an Ovum.—Spencer, in his *Principles of Biology*, has utilized the mathematical law, that in similarly shaped bodies the mass increases as the cube of the dimensions, the surface only as the square, or, in Dr Angus Macdonald's words to this Society, "the mass of spheres of different radii varied as the cubes of their radii and the surfaces of like spheres varied as the squares of their radii." From which it follows that the surface area for absorption of nutritive material and for the excretion of effete matter constantly diminishes relatively to the mass, as a cell increases in size (unless the cell be flattened.) *Conversely*, Balfour, in his work on *Comparative Embryology*, lays down the general law "that the velocity of all segmentation in any part of the ovum is, roughly speaking, proportional to the concentration of the protoplasm there, and that the size of the segments is inversely proportional to the concentration of the protoplasm." He points out that where the segmentation takes place equally, regularly, and rapidly, the eggs are of small size and rarely contain much food yolk. That, in those eggs where there is much yolk, it is usually unequally distributed throughout the protoplasm of the ovum, and that the earliest, most rapid, and most complete segmentation takes place in that part of the protoplasm in which there is least yolk. He takes as his example of this condition the unequal segmentation of the ripe ovum of the frog, in the protoplasm of which he found yolk spherules, those at what he calls the lower pole being larger and more numerous than those at the upper pole where there is more protoplasm, in which are embedded fewer and smaller of these yolk spherules. The first furrows (vertical) always commence at the upper or protoplasmic pole; after the second of these has been formed, a transverse furrow is formed (though the equator of the ovum is much nearer the upper pole), and so on, until the upper half of the egg is composed of a mass of small cells, in which is found little yolk, whilst the lower half is made up of much larger cells, in which yolk spherules may still be distinctly seen. I have also been able to follow out this subject in the ova of the herring and cod through the kindness of my friend Mr George Brook, to whom I am indebted for many valuable hints and suggestions.

In the herring ovum we have, in the first instance, the protoplasm and the yolk mixed together. After a time there is a stream of protoplasm to the upper pole. The separation is not complete, but it is quite sufficient to enable one to see what a great influence the amount of yolk has on the rate of segmentation of the protoplasm. A mass collects at the upper pole; from this threads stream down into the yolk. Here again the segmentation commences at the protoplasmic pole, and there goes on with considerable rapidity; but that part of the protoplasm which is in direct contact with the yolk does not take part in the general segmentation, but remains as a distinct layer of undivided protoplasm for some considerable time—the so-called parablast.

In the cod ovum there is also a streaming of protoplasm to one pole, at the surface, but we have here no threads passing down into the yolk. Segmentation commences at the protoplasmic pole. Here the cells are smaller, but, as we pass from the surface, they gradually become somewhat larger until we reach a layer of protoplasm immediately in contact with the yoke in which there is no trace of division into separate cells. Here it should always be borne in mind that the small epiblastic cells are formed at the protoplasmic or so-called formative pole as well in the human as in other embryos. In *Nassa Mutabilis* we have a still more marked following out of the same law. In this form the yolk, as in the last case, sinks by gravity, the protoplasm furthest from the yolk begins to divide, and the rate of division and size of the cells are in inverse ratio as the amount of protoplasm present in any part of the ovum.

Here, then, we have a statement of a law, or a hypothesis if you will, by Spencer, and following it we have certain facts given by Balfour which may be said to bear out this law. These facts have, I believe, very great importance in pathological processes, an importance which can scarcely be over-estimated; but for the present we will consider its bearing on tumours only.

It is a usual statement that tumours are the result of increased or diverted nutrition to a special area of tissue, and that there is always great difficulty in distinguishing between a neoplasm and a hyperplasia. I hold that the essential feature in the formation of a tumour is that we have in it the result of impaired nutrition: although there may be actually more nutriment going to a part, this may still not be equal to the requirements of the tissue.

Let us see what takes place in a cell when it first comes into existence. For a certain time it takes up food, begins to increase in size; if it be an amœboid cell, vacuoles are formed and extruded. The protoplasm becomes, first, more granular, and then, after a time, the activity of the cell appears to diminish, and the protoplasm becomes clearer. Pseudopodia, which are at first projected, become less frequently thrown out and less prominent, and at length the cell appears to become very sluggish. Should you cool down your warm stage, the cell becomes sluggish much sooner, but should you stimulate the protoplasm by heat, or in any way that may be suggested, all the activities of the cell become increased, nutriment is taken up and effete matter is thrown out. If the supply of food stuff is sufficient in quantity and readily accessible, we can imagine that under a certain stimulation the building up of the cell will go on for a certain period in excess of the breaking down process,—that is, there is anabolism in excess of katabolism. There comes a time, however, at which these two are balanced, if stimulation continues constant. Now, however, increase this stimulation in the slightest degree, and we immediately have a different condition set up, the work which the protoplasm is now called upon to do is in excess of the food supply. There is no longer a condition of balance, but there is an increased demand without corresponding supply. To do the work that it is called upon to do, the cell must undergo some change or other. It cannot perform its full specialized functions at this higher pressure, and consequently it does two things,—it increases its area of absorption, and, at the same time, increases its area of excretion by dividing into two (see above hypothesis). That this is not mere theory we have the above quoted and many other facts to prove, and at some future time I hope to be able to bring some additional facts to bear out Dr James and Mr Geddes's theory from numerous pathological processes, especially in inflammation and repair.

Let us take as an example what takes place on the ovum. We have a cell so placed that it is kept from all excessive irritation; it grows slowly, but constantly, the large supply of food and everything tending to allow of its developing its building up powers to the full; it does not excrete much, from the simple fact that all the energy is devoted to storing up food which is probably brought to it in its most suitable form. So long as this cell remains where it is, so long will its characters remain comparatively unchanged. If now, however, when it is released from its follicle, it comes in contact with a spermatozoon, or what Geddes calls a katabolic cell, the cell in which the activity is exceedingly great,—so great that the tendency is rather towards a breaking down than a building up,—we have an alteration in the condition of affairs. Immediately the protoplasm of the ovum is stimulated to increased activity, and we have in consequence an altered state of nutrition, and the segmentation of the ovum commences in accordance with the above law. Whether the hypothesis be correct or not, we know that this is what actually takes place. This question of nutrition and the relation of irritation to nutrition is constantly coming into play in connexion with tumours, and I cannot lay too much stress on it at the very outset. We find then, where irritation is greatest, and where consequently nutrition is imperfect, that the cells which would under different conditions become to a certain extent specialized—that is, would take on a certain function,—have their energy diverted, as it were, into reproduction instead of becoming developed functionally. It is all they can do to maintain their own existence, or the existence of their species; they have not energy enough to pay attention to their specialized function; for the present they are engaged in reproduction. I have spent some little time on this subject of nutrition, because it is on this I conceive that the greater part of the etiology of tumours must be based. Here, however, another most important law comes into play—the law of heredity.

We have been taught by the cellular pathologist that cells beget their like, and such is undoubtedly a matter of everyday experience. We must, however, add a rider to the effect, that cells beget their like only under certain conditions. We know that by varying the conditions we can to a certain extent alter the character of the cells. Goodsir's description of the behaviour of cartilage cells in the healthy condition and when subjected to irritation is an admirable illustration of this. (This is just a corollary to the above.) We know, too, that if the irritation be removed, the cells which are formed gradually assume the aspect of cartilage cells. So far so good. This law of heredity does not work in one direction only; it comes quite within the range of possibility, that if cells are formed generation after generation under the action of some irritant, that their specialization may be gradually interfered with permanently, and a less and less irritant serves to keep up this new form of cell, until eventually an acquired form may become more or less permanent, especially if constitutional conditions assist—such conditions as those to which we shall afterwards have to refer.

Let us now take up in order some most important facts connected with the development of the different groups of tissues of the body. For our purposes it will be well to group epithelial surfaces together, and to look upon connective tissues also as a single group.

All the epithelial surfaces are formed by a single layer or by several layers of cells, the forms of these cells varying very greatly according to the function which they have to perform. The connective tissue I need not describe, but I may point out that whatever groups of cells we examine, and in whatever conditions, we always find the same rule—the greater the vegetative power the less the functional. We may follow this out during the development of the various tissues. During intrauterine life, when we have development and growth going on at a very considerable rate, and when a great part of the work is done by the maternal tissues, we find that those tissues most rapidly developed are those of the connective tissue group. These tissues are plastically extremely active, but they do not become fully developed; a great part of the energy is diverted from ordinary growth and nutrition to reproduction. Even in those organs in which we have a development of the epithelial structures we find that energy and nutrition are diverted not to function, but to reproduction. It is on this account that we have all the infoldings of epithelial surfaces at certain points, and that we have, as it were, the outline of the structure of the gland long before it is called upon to perform its function as a whole. Even when the gland becomes active, the first draft on its energy is for vegetative work.

During this period we cannot describe either of these groups of tissues as stable, and one can readily imagine that any great nutritive disturbance, especially where impaired nutrition is due to increased demand on the cells, as by an irritation, will give rise to tumour growth if the irritant be local.

What do we find to be the actual fact? Sir James Paget points out that the special tumours met with during fœtal and early life are, in by far the greater proportion of cases, of sarcomatous character. Primary encephaloid cancers are in some instances met with, but these may, in almost every instance, be referred to organs in which there appears to be some interference with the development of the organ. In these cases, I hold that we have as yet an unbalanced condition of the tissues; the building up process is going on at a great rate, and the tumours occur in those tissues—that is usually connective tissues, on which the greatest strain is put under ordinary developmental conditions.

What do we find in adult life? At this period the two sets of tissues may be looked upon as growing equally, side by side. They are both now past the great vegetative strain, and have settled down to their everyday work and to their everyday nutrition. Not only that, but they appear to be, to a certain extent, interdependent one upon the other—an increased growth of one is accompanied by an increased growth of the other. Here again we find—and again I take the authority of Paget and Virchow—the tumours of adult life are adenomas, cystic glandular tumours which have no great tendency to become malignant, the so-called cystic sarcomas and fibro adenomas, in which we have always an increase in both sets of tissues, the glandular tumours of the parotid, etc., and it is a curious fact that these vary in one direction or the other, *i.e.*, are more sarcomatous or more glandular, according to the age at which they are developed. Lastly, we have a stage, of which we may speak as the stage of cancer, where, as it were, the connective tissues have passed their prime, and where the epithelial tissues are also on the turn, but where they have still great vitality, sufficient to be diverted from the imperfect functional to a vegetative activity. It is at this stage that cancers and epitheliomas are common.

Statistics tell us that during early life tumours of the connective tissue type are most frequently met with. In adult life, mixed and frequently non-malignant tumours, and, lastly, in old age, or rather at the end of adult age, epithelial tumours occur. I hold that the same irritant may give rise at these different periods of life to the different tumours, and that because of the different nutrient and developmental states of the tissues. Paget contends that an irritant morbid condition of the blood is the cause of the growth, and he speaks of the constitutional nature of cancer. It must be conceded that the cancerous condition is a constitutional one, but in a far deeper sense than that of which we speak as a blood condition. We must look for it in the actual condition of the tissues themselves.

Let us for a short time confine our attention to epithelial tumours which occur as the decline of life advances. We are accustomed to say that cancers of the breast occur in women from 45 to 50 years of age, as Paget points out, when the functional activity of the highly organized epithelial cells of the milk gland is declining. If there is continued stimulation of these gland cells, it is accompanied by an increased demand on the activity of these cells, which are now neither sufficiently organized nor sufficiently nourished to be able to perform their full function, and as a consequence, vegetative activity increases, and we have formed a number of imperfectly developed epithelial cells.

In classifying cancers and epitheliomas, the age of the patient and the organ affected should always be taken together. For instance, we find that epitheliomas of the skin and mucous membrane (in which function is never so specialized, but remains longer) are usually developed later in life, between 60 and 70, and in connexion with this, I should like to bring under your notice one or two important facts, some of which, no doubt, most of you have already observed. If one examines first the tongue of a fœtus, then that of a child, an adult, an old person of between 50 and 70, one cannot but be struck by the very marked differences in structure. In the fœtus of eight months, the epithelium of the side of the tongue consists of several layers of small cubical cells, on which are superposed one or two layers of slightly flattened cells, the papillæ are few and small, and the submucous tissue of the rest of the body is of a distinctly myxomatous type. Here we have apparently a rapid growth of the connective tissue, whilst the epithelial elements form merely a thin delicate covering. In the tongue of a child two or three years old, or even younger, we find all this altered. The connective tissue, though still growing rapidly, as is evident from the large number of nuclei to be seen, has now a more definite connective tissue structure, the vessels are comparatively large, and the epithelial cells on the surface have the characteristic arrangement and appearance, the cells of the lower layers are cubical or slightly columnar, those above are polygonal, and those near the surface are flattened, and often of considerable size. In the adult there are but two changes from this — the connective tissue layer is perhaps still more definite in its character, is increased in thickness, contains fewer small round cells, though the bloodvessels are just as numerous and as prominent. Coming to the surface, the epithelial layers are simply more numerous, though we have a rather more marked horny layer. On examining the tongue of an old person we are at once struck by the very marked changes that have taken place in both the connective tissue and epithelial layers. The connective tissue layer is distinctly thinned, and at its deeper parts there are very few small round cells; there may be a few near the surface, and of these we shall have more to say immediately. The fibrils of connective tissue are wide apart, and appear to be much atrophied; the epithelial tissue is, however, in quite a different condition. Under a low power we may see the masses of epithelial cells, instead of being simple as heretofore, are now very considerably branched, small finger-like processes passing down from the main mass into the connective tissue layer, until in some cases one could almost imagine one was examining an epithelioma, especially where, as in some instances, there are small cell nests formed in the thickened epithelium. When we have this condition of affairs normally, for it seems to occur in the tongues of most old people, one can readily understand how some comparatively slight but long-continued irritation at this point may give rise to an epithelioma. In such a case we have practically an invasion of the much atrophied and non-resistant connective tissue by the highly vegetative epithelium.

Before this Society it is not necessary for me to refer to the work of Dr Foulis, whose beautiful researches we have all admired. I cannot altogether fall in with his thesis, that it is only the ovum that is derived from the involuted layer of epithelial cells, though there are several most important facts which seem to favour that view, but in connexion with his work I should like to remind the Society that in the ovary of the embryo the invagination of the epithelial layers seems to take place either before connective tissue is formed or before it is present in very great quantity. We have, as it were, a laying down of the outlines in epithelium, and then a growing in of the connective tissue framework.

In old people it has often been remarked that, as involution of the breast proceeds, the connective tissue becomes opener, the spaces larger, the cells fewer; in fact, it becomes atrophied, and this atrophy becomes more marked as age advances. If during this period of atrophy there is no irritation, there comes a time at which the epithelium also undergoes atrophic changes, and there is a steady involution. Should there be any irritation, however, there is a greater instability in the epithelial cells, and as a result we have vegetation or proliferative activity on their part, an invasion of the non-resistant atrophied connective tissue, and the formation of a cancer.

In discussing the ingrowth of epithelium I should like to refer to another point in development. There are, as we know, three segments in which the alimentary canal is developed,—(1), The mesenteron (hypoblast), from which the greater part is formed; (2), the stomatodeum or mouth invagination, which is formed by an ingrowing of the epiblast; and (3), the proctodeum or anal invagination, which is formed by the ingrowing of the epiblast at the other extremity.

As is well known, these two latter are the positions in which epitheliomas make their appearance. This may be partially due to the fact that at these points we have the greatest exposure to various forms of irritation. Making full allowances for this, however, we should still bear in mind that the ancestors of the cells in this position acquired a habit of invasion, and that what Creighton calls unconscious memory may here play a not unimportant part in the process of tumour formation. It was Mr Syme, I think, who insisted that a cancer of the rectum was always within reach of the finger—a fact which throws light not only on the development of cancer, but on the development of the rectum.

Virchow in his great essay on Metaplasia laid special emphasis on the fact that in certain diseases we might have an actual transformation of one kind of tissue into another,—usually, however, to one nearly allied. He holds that although not yet proved, it is quite possible for a carcinoma to be developed from connective tissue. Creighton, we know, enunciates the same doctrine in his work on *Cancer of the Breast.* Whilst holding this view, Virchow conceded that it was a state of affairs comparatively rarely met with. For this statement he was loudly called to account by embryonic layer pathologists, but what is there so extraordinary in his statement? All cells are derived from a single cell, the ovum, and as it divides we cannot for a time make out any difference in the appearance of these cells even after differentiation into layers has commenced.

We have first two epithelial layers, and the mesoblastic or connective tissue only makes its appearance at a later stage still. Once started, it rapidly outstrips the others. Had we taken one of the epiblast cells and placed it amongst different surroundings, might it not have developed into a mesoblast cell? Seeing then that we have these indifferent cells during embryonic life, cells which, owing to their food, their position, or their surroundings, may become developed into epithelial or into connective tissue cells, is it not also possible that under certain conditions we may have first a reversion to the original indifferent cell, and then a partial evolution to one or other of the more specialized types? That such a cycle seldom occurs I should be one of the first to admit, but that it never occurs I should be very chary of conceding. A limited metaplasia—*i.e.*, a modification of tissues of the same group or an imperfect evolution of the cells of that group—is, we know, of very frequent occurrence; and so it is, we say, that tumours are derived from certain embryonic layers. A sarcoma or tumour of the connective tissue type from the mesoblast,—cancerous tumour from the epi- or hypo- and mesoblast. In discussing the constitutional aspect of tumours, I spoke of the age of the tissues as one of the important factors in the production of tumours. As we are told in a certain department of this School, "Gentlemen, we are not as old as our years, but as old as we are."

A man may have his age passed on to him from old parents, and the most interesting question to be determined is, In what proportion of cancerous patients is this the case?

His tissues may become aged from bad surroundings, want of food, want of air and light; we may have ageing of tissues, on the other hand, from imperfect nutrition, due to excess of food, a very good example of this being in the case of gouty individuals (hereditary age manifesting itself in the same disease). Syphilis, malaria, and various other diseases are all most potent factors in the conditioning of tissue old age, and all are said to play a part in the causation of tumours.

In conclusion I may sum up as follows,—Imperfect nutrition is the cause of cell proliferation. Imperfect nutrition may be due to a want of food, to an increased demand for food set up by an irritant, or by an increased difficulty on the part of the cell owing to its size and position of taking in food and of getting rid of its waste products.

During the life of the individual, we have the tissues preponderatingly anabolic or katabolic at different stages of that life. The curves corresponding to these do not correspond to, nor do they run parallel to, one another.

During fœtal and early life the anabolic curve is high, all the tissues are growing; the katabolic curve is also very high, correspondingly higher in the connective tissues than in the epithelial, with one or two exceptions before mentioned. As life advances we get a more stable condition, the anabolic curves remain higher and more evenly balanced in the two sets of tissues, the same with the katabolic curves. But at the end of adult life we find that the anabolic curve falls in both sets of tissues, a gradual decline in that of the connective tissue, but first an abrupt drop in that of the epithelial tissues in the female in connexion with the involution of the breast, etc., and then the more gradual one of the skin, etc.; in the male we have only the more gradual falling. Towards the end of life the epithelial curve does not fall so rapidly as the connective tissue curve.

It is during the periods that these curves become separated from one another that we have the formation of malignant tumours, and then because of the demand on the resources of the tissues being in excess of their nutrition.

The irritant giving rise to this increased demand may be a simple injury, or it may be found to be a parasite, or microbe in some instances, or a long-continued action of some irritant, organic, or inorganic matter, or it may be merely a chronic catarrh.

These irritants can only take full effect and give rise to a tumour when the nutrient conditions of the tissues are so altered by irregular nutrition or age that the line of resistance is passed, and a tumour hitherto composed merely of granulation tissue in which there is a tendency to higher organization loses part or the whole of that power, and remains a sarcoma. Or a tumour hitherto composed merely of granulation tissue covered with epithelium becomes a tumour of non-resistant granulation tissue, into which epithelial processes can make their way, every day increasing the facilities for the continuance of the condition.

Prof. Chiene said,—Dr Woodhead has considered the etiology of tumours from the pathological standpoint. I desire to consider the same from a clinical standpoint. Mr Syme used to point out that patients often referred tumours to a blow, and used to warn us that this idea was to be received with caution. He pointed out that patients do not wish to acknowledge a constitutional taint for any malady, and when a tumour arises hunt back in their memories for some injury which will account for the presence of a tumour. As we shall see, to accept a blow as the starting-point does not invalidate the idea that there is a constitutional predisposition. E. B., aged 55, came under my care in June 1879. She suffered from a fibrous tumour on the outside of the thigh, and stated that she received a severe blow from the falling of a mangle in February 1879. It left a mark, and the swelling which then formed never disappeared. The tumour which existed was in the exact position of the original blow. It was a recurrent fibroid, and after repeated operations the patient died of sarcomatous tumours in her lungs and kidneys in 1884. This case made a strong impression on my mind, and since that time I have seen several cases which have strengthened me in the opinion that, while tumours often arise after a chronic irritation and in the seat of an old injury, in some cases they have a distinct acute traumatic origin. This is the opinion of most pathologists. Erichsen says (vol. i. p. 965), "Local injury or mechanical injury is undoubtedly the determining cause of the growth of the tumour in a certain proportion of cases. . . . . The effects of this cause also are most marked in malignant tumours. Different authors state that these causes are observed in from 7 to 14 per cent. of all the cases." Paget says, "All the facts relating to injuries in favouring or determining the growth of tumours are explicable on the supposition that the injury impairs for the time the nutrition of the part, and diminishes its power of excluding abnormal methods of irritation," p. 389. In speaking of sarcomata, he says, the influence of injury is clearly shown in certain cases. In speaking of epitheliomata, he says injury by violence appears as the exciting cause. Ziegler, however, says that "in consequence of the essential difference of neoplasms and inflammatory swellings, we exclude at once the possibility of attributing a tumour to a traumatic lesion, at least in any immediate or direct way." He adds, "that clinical experience bears this out." With this I cannot agree. He then says, "If we have tumours developed in a tissue that has been injured, it is, on the whole, a rare occurrence, and does not prove that such injury would of itself suffice to set up tumour formation in a previously healthy tissue." I do not believe that a tumour would arise in a healthy tissue. If the tissues are previously healthy, then he is quite right in the opinion that a traumatism cannot be the starting-point of a tumour. I am of opinion that the local manifestation is always due to a local irritation, lowering for a time the resistance of the tissues locally, and rendering the irritated part liable to the tumour formation. I know nothing of the predisposing cause which renders some people liable to tumour-growths, but it must be something of a very general nature—probably a blood disorder—the blood being the only tissue common to all the organs; and if this be granted, there must be some local alteration to account for the local manifestation, such as would be caused by an injury,—using that word to include chronic irritations, scar-tissue from an old injury, and acute traumatic injury, such as a blow, fall, or strain. Cohnheim's theory of a belated embryonic nodule is not opposed to this view, for it necessitates something local to start the active growth. It has to be noted, that if his theory is correct, then these nodules must be very general throughout the body, tumours attacking almost every organ and tissue. We must be made of belated embryonic nodules. The general predisposition, whatever it may be, is local as regards time, because we know that certain tumours are met with at certain ages. That vascular unrest is associated with tumour formation is probable, when we observe the frequency with which they occur in organs which, as regards their vascular supply, may be called tidal organs, such as the mamma, ovary, and testicle. The alterations in the vascular supply, the periods of comparative functional inactivity alternating with periods of excessive functional activity, associated with an increased blood supply, all point to alterations in the vascular supply as having an important bearing on this subject. The one thing common to all injuries is an alteration in the blood supply of a part. This consideration further strengthens the view, that a local injury is often associated with the formation of a tumour. The tissues themselves, looking to the whole life-history of the individual, are also tidal. The connective tissues are active at one period, the epithelial at another period in the life of the individual. This may have a bearing on the fact that certain tissues are more liable to tumour growth than others at certain periods of life. What contribution to this view can I bring from statistics? Mr Wallace has examined the hospital records of Mr Syme and Mr Spence, and has found that of 200 consecutive cases of these surgeons, 39 were ascribed to a blow or irritation. In my own practice Mr Musgrove has tabulated 238 cases: 87, or 36 per cent., were ascribed to a local irritation or injury; and 31, or 13 per cent., to direct traumatic injury. Of these 31, 9 were sarcomata, 14 carcinomata and epitheliomata, and 8 simple tumours. My argument, stated shortly, is, that there is a general predisposition, only local as regards time; and that if a person so liable is injured, the essence of an injury being a lowering of the power of resistance of the tissues to external irritants, at the seat of injury a tumour arises. Statistics are brought forward to show that because tumours rarely arise from blows, therefore traumatism cannot be accepted as of primary importance. I object altogether to the rarity of these cases being used as an argument against the idea that a local irritation is an essential to start the local manifestation. It only shows that the acute traumatic form of irritation may be an uncommon starting-point. These comparatively rare traumatic cases must be accepted. Their acceptance in all probability shows that in those cases in which no cause can be assigned the local irritant has been overlooked. It need not be traumatic, and is therefore not within the cognizance of the individual; and in many cases in which no cause is assigned by the patient, the part may, however, have been injured, some local imperfection taking place in its functional activity, as in an inflammatory area in the mamma in suckling, which remains below par, and afterwards, when the predisposition arises, becomes the seat of tumour formation. I argue for the local irritation as the exciting cause of tumour growth in people predisposed to tumour formation. I know nothing of the exact nature of the predisposition.

Dr James had no hesitation in saying that the interest attaching to Dr Woodhead's paper was intensified by the fact that it was a new departure as regards the work he had been doing. It showed that he had here made a generalization which was an important one. It was a mistake if a man limited himself entirely to a particular work, and did not now and then draw together the tangled threads and arrive at some conclusion from them. No one had a greater respect than he had for detailed work, but detailed work alone was like the foliage of a tree, it obscured the main stem. From a pathological point of view, the theory he had brought forward seemed to him in the highest degree justifiable. He (Dr James) was specially pleased to meet with it, because it was similar to views he had himself held for some time, and which were known to the Society. Pathology taught them that a tumour was a new growth, differing in type from the tissues in which it grew, but yet resembling the tissues of the adult or of the embryo. Of course, in tumours they might find degenerations, fatty or colloid, or inflammation, ulceration, and gangrene, but that did not affect this general conclusion. Some of the tumours were malignant, some benignant. By malignant tumours they meant tumours which had the greatest power of selecting for their own nutrition material which should go to the individual. A malignant tumour did this by extending rapidly, involving other tissues, or by metastasis. A further point was this, that in the main, tumours which resembled the tissues of the adult were not so malignant as those resembling the tissues of the embryo. Of course they might have apparent exceptions. An adenoma, for example, in the skin grew slowly, in the alimentary canal rapidly. Such in the main being the data pathology offered, let them see what information they could get from the study of biology. They knew that in the ovum after impregnation there was segmentation,—until the blastoderm was formed. This split into the three layers—epiblast, mesoblast, and hypoblast—and from these three, they had in time the various differentiated tissues produced. Let them now consider the proportionate activity in the metabolic processes at the various periods of life. They knew that every organism showed an anabolic ascent, next a balance, and lastly a katabolic descent, and that the proportionate activity of the anabolic and katabolic processes in the embryo was much greater than at subsequent periods of life, just as in the child it was much greater than in the adult. Thus the child does not assimilate and excrete so much absolutely as the adult, but in proportion to its weight it assimilates and excretes much more. In the child the tissue metabolism is more active than in the adult, in the embryo it is more active than in the child. From this physiological standpoint they could easily understand why a tumour resembling an embryonic tissue should be malignant. Suppose such to occur in the tissues of an adult. Its metabolic activity being great, it at once begins to assimilate and excrete to a degree immensely in excess of the other normal tissues—that is to say, it increases with great rapidity at the expense of the individual, it is able to rob the normal tissues of their share of nutriment. This had been shown experimentally. If fœtal and adult cartilage were implanted in adult tissues the fœtal cartilage would continue to live much longer than the adult. The more like the tumour was to the earlier embryonic tissues, the more malignant would it be. One could easily understand from this why the tumours like the embryonic tissues should be very malignant. How did a tumour occur in the adult? They had all heard of Cohnheim's theory of the belated rudiment. There was something to be said in favour of it, but was there any necessity for the supposition of such belated rudiments in the adult body? There were always tissues that were in an embryonic stage. Connective tissues were constantly wearing out and being replaced by new tissues. The new tissues were formed by cell-development from the old. These cells passed through the same stages as in the embryo, but much more rapidly. Given at any moment in their earlier stages a defective nutrition or an irritation, what they found was that in obedience to the law of the antagonism between nutrition and reproduction, these cells, instead of nourishing themselves and differentiating so as to form an adult tissue, reproduced themselves, and so formed an embryonic tissue, *i.e.*, a tumour. There was no need for "belated rudiments." This theory would also explain simple tumours. Let them suppose, for example, that there were formed in the adult, fat cells with a metabolic activity like that of the child or the fœtus, then a lipoma would be produced. The theory would also enable them to understand inflammation. Given a severe irritation, they had a tissue formed by a process which was the furthest back in the embryonic stage —a simple formation of cells. In this an analogy was presented between inflammation and tumour growth.

Dr Duncan said it appeared to him, in listening to the paper by

Dr Woodhead and the remarks by Prof. Chiene and Dr James, that they had not laid sufficient stress on the fact that they were dealing with two different subjects. From the clinical standpoint simple and malignant tumours were etiologically and teleologically different diseases. The one was developmental, the other was irritative and intrusive. The one had its analogy in teratology, and the other in inflammation, tubercle, and syphilis. He could understand that Cohnheim's theory might apply to innocent tumours. He did not believe it did so altogether. It made too great a demand on their credulity to predicate so many belated individuals throughout the body. Neither did he believe with Drs James and Woodhead that tumours must be the result of defective nutrition. Why should a fatty tumour grow because its nutrition was defective? You may check or kill a tumour by diminishing its blood supply. A simple, innocent tumour was non-intrusive, and might be the result of an excessive blood supply. The malignant tumour, on the other hand, was intrusive, and he had the greatest possible sympathy with those men who were making efforts to find the microbe of malignant disease. He believed that malignant tumours should be considered zymotic in character. If they considered the analogies to inflammation, to syphilis, to tubercle, they saw this. How difficult it was even for the microscopist to distinguish between these! There was no boundary-line histologically between them. He had at present in his wards a case of sarcoma of the ear and lobe, which commenced as an eczema, and was treated for a long time as such. There were many similarities in their behaviour in the intrusive character of the processes. It was interesting from this aspect to look on the way in which granulations would eat out a portion of bone. He looked on the tissues of the body as in a state somewhat like that of the balance of power in Europe. When they spread their boundary it was under the influence of microbes. They had found that in tubercle the cause of the disease was a microbe. They were certain that it was so also in syphilis, though they had not found it; and he believed that in malignant tumours they would find a microbe also as the cause.

Mr Joseph Bell said that thinking over this subject from the point of view of the practical surgeon, one came to see that in the case of some kinds of tumours they at once expected to find a traumatism as the cause. A great many fatty tumours were due to some kind of irritation. In his experience a very common situation for such was the shoulder, and the joiner's trade was the one which supplied most patients affected in this way. For many years he had held the view that there was always a distinct cause for the occurrence of epitheliomatous growths. For example, a history of phymosis was almost always given by sufferers from epithelioma of the penis, and cancer of the lip rarely appeared in non-smokers. The consensus of opinion among the lower orders in regard to scirrhus of the mamma was that there was always a preliminary injury to account for it,—a bad breast during nursing or a blow. His own experience was the opposite. He had rarely been able to be sure that an injury had anything to do with it, and would be inclined to give the lactation changes and inflammatory occurrences as more likely causes. Sarcomata, especially of the bones, in young people had frequently a traumatic origin. An injury causing the separation of periosteum from the bone and effusion was not unfrequently the starting-point for a rapidly growing sarcoma. Mr Butlin recorded several cases of this kind some time ago, and they were well known to surgeons.

Dr MacGillivray thought it was an easy matter to start new theories, or consider theories that had been started; but it seemed much more difficult to come to a conclusion. As to the diathesis, there was the view that there must be something behind the irritation which caused the tumour growth. A number of theories had been put forward to explain the diathesis. It seemed to him, however, to be very similar to what they had in struma or syphilis —a condition of low vitality. He thought there might be in certain individuals a tendency to the development of tumours on certain irritations. As to the special form of irritation, various theories had been put forth. The idea of lower organisms was first brought forward by Hueter ten years ago. It had been supposed that goitrous growths might be caused by the presence of such organisms in the drinking water, Klebs having discovered navicula in the waters of such districts. An interesting question in connexion with the further development of tumours was what led to the formation of one kind at one time and another kind at another. In the same individual they might have a round-celled sarcoma followed by a spindle-celled, and afterwards by a fibrous tumour; or the reverse process might take place. Was it possible that the cause of this difference was to be found in the kind or degree of the irritation that set up the tumour process?

Dr Russell said he could not approach this subject from the standpoint which had been taken by the surgeons who had spoken, so would confine himself to the consideration of the subject as presented by Dr Woodhead. With the surgeons he agreed that simple tumours might, to a certain extent, be regarded as overgrowths. Passing from them to malignant tumours, Dr Woodhead, in the latter part of his paper, referred to a doctrine associated with the name of Cohnheim,—that known as the antagonism of the tissues. Mr Duncan also referred to this view in comparing the state of the tissues to the balance of power in Europe. To his mind it was more proper to say that the tissues maintained a state of nutritive equilibrium towards each other, which was different from the idea represented by the term mutual antagonism. Cohnheim's doctrine was worked out in Dr Woodhead's paper. What influence had it on tumour formation? In old age Dr Woodhead argued that fibrous tissues became wasted, and epithelium asserted itself. As a result, it was expected that epithelial tumours should be produced in old age. If this assertion were correct, and had a bearing on tumour growth, would they not expect to find epitheliomatous tumours produced in such regions as the palms and soles. This was not the case. Further, the carcinomata were not confined to old age. Look also at a primary carcinoma of the liver or kidneys, or a primary sarcoma of either. Was it to be assumed that there was an assertion of the secreting epithelium in the one and of the connective tissue in the other? Was scirrhus cancer to be regarded as an assertion of fibrous tissue or of epithelium? Dr Woodhead regarded scirrhus as an epithelial structure, yet every one knew that there was no lack of fibrous tissue in scirrhus. He said nothing of certain French views, which regarded it as a fibrous tumour. As to blood supply, Dr Woodhead and Dr James laid down the law that tumours were the result of impaired nutrition. This was based on the developmental theory, that cells subdivided when called on to do extra work. As Mr Duncan had pointed out, this was a monstrous law to apply to tumour growth; and Dr James contradicted himself, for he said that malignant tumours nourished themselves at the expense of the individual, and led to exhaustion and emaciation by so doing. There was no want of nutrition, therefore, according to Dr James's own showing. There was the other view of Cohnheim, that of belated rudiments, of which he would say nothing at present. There was also the view that micro-organisms were the cause,—a view he was surprised to find the surgeons advocating. His own feeling was that if they knew anything at all of the action of micro-organisms, he should expect that malignant tumours would not be caused by them, at least the carcinomata would not. In considering malignant growths there were two fundamental questions—Was it a blood condition or a local condition? All pathologists were familiar with Paget's exposition of Treviranus' law, that every tissue was in relation to other tissues to be regarded as an excretory organ, and he supported this with that characteristic completeness and persuasiveness of logic that one felt much tempted to accept the constitutional view. But there was the other theory, that of local irritation. It was well known that malignant tumours were sometimes traced to some injury or irritation, and the irritation might be such a common one as smoking, or decayed teeth; but they also occurred during periods of functional activity. It was important to bear in mind the vast amount of negative evidence. There were vast multitudes of smokers who did not develop cancer of the lip, and great numbers whose teeth were jagged did not have cancer of the tongue. What did they know of irritation? They knew that in certain cases friction led to the production of a psoriasis. They said this was due to the constitution—some chemico-vital condition which expressed itself both in the tissues and the blood. They thus got back to Paget's view. As soon as ordinary irritation was overcome or removed, the embryonic tissue which was formed during the process of irritation disappeared or tended to revert to the type of the tissue which was its matrix. Malignant growths were entirely different. In cancers, if there was not a return to embryonic form there was to embryonic activity, which was vastly more important. In cancers the cells maintained their epithelial character right through. It was only in some of the sarcomata that there was a persistence in embryonic type. In addition to this the cells were able to carry with them this power of reversion to embryonic activity when carried to other parts. This was quite distinct from inflammatory conditions. It would appear that this was an additional argument in favour of the view that there was a law in the tissues of tendency to reversion to embryonic type and activity. This was similar to Creighton's unconscious memory. It was really a second childhood of the cells.

Mr Miller said Mr Chiene had dismissed Cohnheim's theory with the remark, that if it were true we would require to be formed of these belated individuals. Not so. It was simply necessary for the truth of this theory that there should be a belated nodule in the tissue in which the growth took place. He had been staggered by Dr Woodhead's statement regarding the epithelium of the tongue in aged persons frequently containing epithelial nests, though there was no apparent disease. The impression made upon his mind was that if this were true, then the removal of the tongue for epithelioma from a person above a certain age would be almost hopeless, on account of the great liability of the disease to return. So far as he could see, every speaker that evening had admitted irritation as an important cause of tumour growth.

Dr James Ritchie thought that Dr Woodhead, in discussing this question, had done rightly to consider cell growth; because on the conditions which regulated the nutritive and the formative power of the cell depended the growth not only of malignant, but also of simple tumours. He did not think that the proposed theory of impaired nutrition accounted for certain facts in the history of these growths. The power of independent growth seemed to be possessed in a remarkable degree by the cells of certain tumours. No explanation was by it offered of the indefinite growth of tumours generally, nor of the determinate growth of others, such as painful subcutaneous tumours and exostoses on the phalanx of the great toe, nor of the arrest of growth of some tumours for a time, with again an increased activity without known cause. The infective power of the cell had not been touched on.

Meeting II.—December 7, 1887.

Dr John Smith, *President, in the Chair.*

I. Election of Ordinary Members.

The following gentlemen were elected Ordinary Members of the Society:—Thomas W. Dewar, M.B., C.M., Edinburgh, and D. H. Anderson, M.B., C.M., Edinburgh.

II. Exhibition of Patients.

1. *Dr John Thomson* showed a case of myxœdema. The patient, unmarried, aged 51, came under his care about five weeks before, complaining of want of energy and strength, and a feeling of coldness in her extremities, with a dryness of the skin all over the body, also of weak digestion and constipation. These symptoms commenced about nineteen years ago, and increased slowly but steadily ever since. When they began she was suffering from leucorrhœa and menorrhagia, which, however, was not great, and lasted for about a year. At that time, and ten years later, she suffered a good deal of annoyance and distress from family matters, and she thought this had a bad effect on her health. She presented the characteristic features of the disease, the peculiar physiognomy and complexion, the swelling not limited to dependent parts, and not pitting on pressure. The thyroid gland could not be felt. Her hands presented the so-called spade-like appearance. A ring which fitted her ring-finger at commencement of illness was now very tight when placed on her little finger. Her temperature was abnormally low—96°·2 in the mouth when last taken. Her movements were slow. Her general sensibility was good, and not at all delayed. Smell and taste were very good; sight and hearing somewhat deficient. The voice was distinctly affected.

2. *Prof. Grainger Stewart* showed a case of myxœdema. The patient was married, and aged 32. She presented in a very typical way the features of the disease. The treatment that had been tried for her in hospital was arsenic with Turkish baths. A certain amount of benefit seemed to follow the use of the baths, but as she had the signs of mitral stenosis she was obliged to discontinue them. After leaving the Infirmary she hit on a method of treating herself, which seemed to have been useful. Rough towelling, brushes, and so on she did not consider hard enough for application to her skin; but a vegetable grater, steadily applied by herself, has been effectual. A distinct degree of improvement has taken place, not only in her appearance, but her speech, intelligence, her force of will, her memory. He expressed the wish that Dr Thomson, who had been attending the patient recently, should give some account of what he had seen.

Dr John Thomson said that Prof. Stewart's case was an intensely interesting one from a therapeutic point of view. She had been very little better after treatment in hospital; but by a wonderfully energetic use of simple means in her own home, she had wrought such an improvement in her condition that she was now relieved of almost all inconvenience for the time being. She had been under his observation since she left the Infirmary, and her progress had been steadily downhill till the middle of last September. She said at that time that she was worse than she had ever been, and she was certainly a very severe case of the disease. She then began the treatment Prof. Stewart referred to, the details of which were as follows:—On wakening in the morning she massaged the whole of her body most energetically for a considerable time, then rose and exercised all her joints systematically; next she sponged herself, from the waist upwards, with cold water, using a flesh-brush till she felt warm; then she did her household work. In the middle of the day she took a warm bath, using the fleshbrush again; and, as Prof. Stewart has mentioned, a vegetable grater, till her skin was sore. In the afternoon she walked for about two hours in the sun, trying to make herself perspire. In addition, twice a week, she improvised hot-air baths as follows:—She made as large a fire as her small grate would hold, took a warm bath before it (as on other days); then in a thin night-dress lay on a couch as near as possible to the fire, and turned herself round till she began to perspire. This was usually in about an hour. She then drank cold water to encourage the sweating, which, if she was not interrupted, usually went on for an hour or even two. Throughout the day she employed her spare moments in massage and moving her joints. He had seen her just before this systematic treatment was commenced, and again after she had five weeks of it. The following facts would help to indicate the amount of improvement during that time. Before treatment there was very great swelling of her face, body, and limbs. After it her face was so diminished in size as to be scarcely recognisable; her clothes had to be taken in 8 inches at the waist and 3 inches at the neck; her boots and gloves were much too big for her. Before treatment her movements were extremely slow and accompanied by severe pain. She was quite unable to stoop to clean her hearth, but had to go down on her hands and knees to do so, and after had to crawl to a chair to help herself up. She could not knit, or sew, or peel potatoes. After treatment she was more active than most women, could go through any ordinary movements quickly, easily, and without pain, and could sew, etc., easily. Before treatment she spoke with great slowness and much stammering, and ideas seemed to come very slowly. Her memory was extremely bad. After it she spoke rapidly and much, and her memory seemed perfectly good.

3. *Dr Affleck* showed four photographs of a patient suffering

from MYXŒDEMA. Two of them were taken at the time of her admission to the Infirmary. They showed in a typical way the features of the disease. The other two were taken three months later, and showed the improvement that had been effected. The treatment consisted of hot baths daily, and the internal administration of iron and arsenic. That these drugs were of benefit was shown by examinations of the blood, the corpuscular elements of which increased in quantity while they were being taken. There was no trace of a thyroid gland in this case. Temperatures were taken every four hours night and day, and never once during the time she was under observation was there a rise to the normal. The hot baths had no effect in raising it, but they added greatly to the patient's comfort. It was of interest in this connexion to recall Victor Horsley's experiments on animals whose thyroid glands had been removed. Depression of temperature was always a result of such removal; but Horsley found that a heated atmosphere made the animals more comfortable, and prolonged their lives for about six months, though they always eventually died of marasmus.

4. *Mr A. G. Miller* showed a boy on whom he had operated for TALIPES EQUINO-VARUS. He had had his tendons cut by Mr Bell in infancy, but from various reasons his parents had neglected to properly apply the boots that had been provided. A wedge-shaped portion of skin, along with a bursa that had formed, and a wedge of bone, were removed from the outer side of the tarsus, the tendo-Achillis cut, and the foot brought down. The result had been a successful one. It was Mr Bland Sutton, he thought, who first drew attention to the occurrence of spina bifida occulta along with congenital talipes. This patient had that condition, and every case of talipes he had examined since had the same.

III. Exhibition of Surgical Appliance.

Mr Cathcart demonstrated for Professor Annandale a new appliance for supporting the nasal bones or septum in cases of fracture or operation. It consisted in fitting over the nose an arch of sheet-lead, transfixing the lead and tissues of the nose with a needle, and then supporting and fixing the whole by a loop of silver wire, which was secured over the arch of lead. An additional advantage of the lead was that it could be moulded to the nose so as to exercise a proper lateral pressure, and so shape the organ.

Professor Annandale was indebted to Mr Cathcart for the cast of the face illustrating the appliance.

IV. Exhibition of Charts.

Mr A. G. Miller showed two charts of EMPYÆMA cases. The first illustrated the value of a mode of treatment suggested by Dr James during the recent discussion on empyæma, viz., very frequent tappings, which would give almost the same results as a free drain without any risk to the patient. The case was that of a man aged 44, a locomotor ataxic, the anæsthetic state of whose skin greatly facilitated the tappings. On the first occasion 75 ounces of purulent fluid were removed from the left pleura. The next day 34 ounces were drawn off, and on the two following days 15 and 8 ounces respectively. On the fifth day nothing was got, and the lung was observed to have expanded. Thereafter a blister was applied, and on the fifth day there was a rise of temperature and evidence of the accumulation of fluid. Tapping was performed, and 20 ounces of sero-sanguinous purulent fluid removed. Four days later 10 ounces of sero-fluid was withdrawn, and five days afterwards nothing was got when tapping was performed. The temperature curve during the course of treatment remained a normal one, excepting the rise after the application of the blisters. The left side of the chest was now quite healthy. The second chart showed the curve of an empyæma case, treated also successfully, but in a different manner. Free incision was performed, but, fortunately, adhesions had occurred before this was done. The lung, therefore, did not collapse, and the patient has gradually recovered. The most interesting point in his case was that there was a complete fault as to diagnosis. He was not supposed to have empyæma at all. After the empyæma was made out, the quantity was falsely estimated at 5 or 6 ounces, while at least 70 were obtained. It was on the right side, and perhaps some of the dulness due to the empyæma was attributed to the liver. This latter case was a very favourable one for an adult, but took three months before recovery was effected, whereas in the former case recovery took place in as many weeks.

V. Exhibition of Instruments.

Dr Maxwell Ross showed a couple of REVERSIBLE NASAL SAWS made for his colleague, Dr Hunter Mackenzie, by Mr Young. They were an improvement upon the saws in previous use, in that they were made so that they could be set at any angle, and their reversibility allowed of their being used to saw upwards or downwards as the operator desired. The serrated edge was not carried all the length of the instrument, in order to avoid the fraying which was apt to occur at the entrance to the nostril when this was done.

VI. Exhibition of Pathological Specimens.

Dr Alex. Sinclair showed a specimen of RUPTURE OF THE LEFT VENTRICLE taken from the body of a female inmate of Craiglockhart Poorhouse. She was 73 years old, had not shown previously any signs of heart disease, and died suddenly when sitting at the fireside. There was a large development of fat over the point of rupture, no doubt associated with disease of the coronary arteries, and there was also a dilated aorta.

VII. Original Communications.

1. ON CERTAIN PHYSIOLOGICAL VARIATIONS IN THE SHAPE AND POSITION OF THE LIVER.

By Johnson Symington, M.D., F.R.S.E., F.R.C.S. Ed., Lecturer on Anatomy, School of Medicine, Edinburgh.

The principal object of this communication is to direct attention to the changes in the position and shape of the liver resulting from variations in the condition of neighbouring organs, more particularly the stomach. Before proceeding to the examination of this question, however, I think it advisable to make some preliminary remarks on the general form of the liver. This is the more necessary since most of our anatomical text-books still describe this organ as having a configuration which differs very materially from its natural condition. The liver is usually said to have two surfaces—one superior and convex, and the other inferior and concave; and four borders—a right and a posterior, which are thick, and a left and an anterior, which are thin. Its dimensions are given as 10 to 12 inches from side to side, 6 to 7 inches from before backwards, and about 3 inches from above downwards in its thickest part, this being near the posterior border of the right lobe.

This description is based upon the appearance of the liver after its removal from its natural connexions, and when flattened and spread out upon a horizontal surface. A consideration of the results of the physical examination of the liver in the living body will convince you that this description differs very materially from the natural configuration of the liver. Indeed, the ordinary account of the shape of the liver, still disfiguring so many of our text-books, is not only anatomically incorrect, but is so useless and misleading to the clinician, that the sooner it is generally abandoned the better.

Although few English text-books give any hint of the alterations in the natural shape of the liver due to the methods usually adopted in its examination in the dissecting and post-mortem rooms, yet Luschka, in his classical work *Die Lage der Bauchorgane*, 1873, and also in his *Anatomie des Menschen*, directed attention to the fact that the removal of the liver from the body was liable to cause great changes in its form. Since this is the case, it is not surprising to find that the results obtained by the examination of livers hardened *in situ* differ greatly from those gained by ordinary dissections.

It is to Professor His,[1] of Leipzig, that we are indebted for the

[1] "Über Präparate zum situm Viscerum, u.s.w.," *Arch. f. Anat.*, 1878.

most complete and definite contributions to our knowledge of the normal shape of the liver. He determined the form and relations of the thoracic and abdominal viscera by injecting the undissected subject with a ½ to 1 per cent. solution of chromic acid, and taking casts of the organs in their natural position after they had been hardened by the chromic solution. The model of the liver I now show you was prepared in this way by Herr Steger under the superintendence of Professor His. It demonstrates the important fact that the liver presents impressions for all the abdominal organs lying in contact with it. This moulding of the liver by the neighbouring viscera, although of great anatomical and physiological interest, is generally ignored in our modern text-books; yet His shows that Vesalius[1] was well aware of the fact, and he asserts that the description and illustrations of the configuration of the liver given by Vesalius in 1543 are more accurate than those of the majority of modern authors.

Although it is necessary to adopt some means of hardening the liver in position, such as freezing or injecting a solution of such hardening agents as chromic acid, in order to make a satisfactory investigation of its shape, yet, with moderate care, you can easily satisfy yourself by simpler methods that His's model is a correct representation. Thus the liver I now show you was carefully removed from a body at an ordinary post-mortem examination. It was at once fixed in its natural position with plaster of Paris and wadding, and then hardened in spirit, so that it can now be freely handled without losing its shape. You will observe that it closely resembles His's model.

His, in the paper already referred to, pointed out various errors in the usual account of the liver. He proved that certain portions of the organ, such as the lobulus Spigelii, the groove for the inferior vena cava, and the posterior part of the longitudinal fissure, although generally described as being placed on the under surface, are really situated on its posterior aspect.

The liver, as represented in His's model, differs greatly in its general form from that commonly described, and he considered that the liver ought to be regarded as an organ with three surfaces rather than two, the additional surface being a posterior one. In my work on *The Topographical Anatomy of the Child*, p. 38, I advocated a more extensive departure from the usual description of the form of the liver, and I believe that you will find the plan there recommended useful in the clinical investigation of the condition of the organ.

From an examination of His's model, or of the hardened liver I have just shown you, you will see that the liver has the shape of a right-angled triangular prism with the right angles rounded off. Its surfaces are five in number, which from their position may be named anterior, posterior, superior, right and left inferior. The

[1] "De Corporis humani fabrica," Liber v., cap. 7.

upper surface is generally convex and moulded to the diaphragm, but it has a shallow concavity where it lies below the heart. The right surface terminates below in a well-defined border, but above, in front and behind, it becomes continuous with the upper anterior and posterior surfaces respectively by rounded borders. Since the superior and right surfaces are continuous with one another by a rounded border, it is impossible to define precisely the extent of each. If, however, a line be prolonged horizontally outwards from the highest part of the upper surface, and another be prolonged upwards from the most prominent part of the right surface, we can obtain a fairly accurate idea of the length of these two surfaces by measuring from the point where these two lines intersect to the left edge and to the lower edge of the liver. An examination of a number of specimens in this way will show that the right surface may be as long or even longer than the superior, and that, in any case, the area of the liver looking to the right side is sufficient to entitle it to be called a surface rather than a border. The surface generally called inferior may, I think, be more appropriately named the left inferior, as it is important to remember that it does not look directly downwards, but also towards the left. The relation of the three surfaces just described to one another is well demonstrated by coronal sections of the liver made in front of the vertebral column (see Woodcuts, Figs. 2 and 3). From these figures it will be seen that the three surfaces, viz., the right, superior, and left inferior, are separated by three borders, a left and a right inferior, both of which are sharply defined, and a right superior, which we have already described as being a rounded one.

The surface of the liver visible on an inspection of the organ from the front or from behind has the shape of a right-angled triangle, the right angle, which is between the superior and right surfaces, being rounded off, and the hypothenuse inclining from the left side downwards and to the right. The anterior surface is generally described as a part of the superior surface, and this is said to terminate in a sharp anterior border. Such a description I consider to be anatomically incorrect, and for clinical purposes misleading. There is a considerable area of the liver looking directly forwards and lying in close contact with the inner surface of the anterior abdominal wall. The so-called anterior border is the well-defined margin which separates the anterior from the left inferior surface. The anterior surface becomes continuous by a rounded border with the right surface, but above its separation from the superior surface is fairly well marked. Thus in and near the middle line there is frequently a distinct border between the upper and anterior surfaces,[1] although, as it is traced towards the right side, it becomes less distinct.

[1] See Plates I. and II. of my work on *The Topographical Anatomy of the Child.*

The posterior surface has been described with great care and accuracy by His. It includes the notch for the œsophageal end of the stomach, the posterior part of the longitudinal fissure, the lobulus Spigelii, the groove for the inferior vena cava, the depression for the supra-renal capsule, and a part of the right lobe above the kidney, which is uncovered by peritoneum, and lies in direct contact with the diaphragm. It can easily be seen from His's model that this posterior surface is smaller, and certainly not better defined than the anterior one. His regards the renal impression as belonging to the inferior surface. In many livers it seems to me that it might more naturally be considered to be a part of the posterior surface. Fig. 1 represents a sagittal section of the body of a boy about six years old, one inch to the right of the middle line. In this case the renal impression looks more backwards than downwards. The anterior part of the liver was compressed by a distended transverse colon, and the anterior surface was thus diminished in size, but it is still quite distinct.

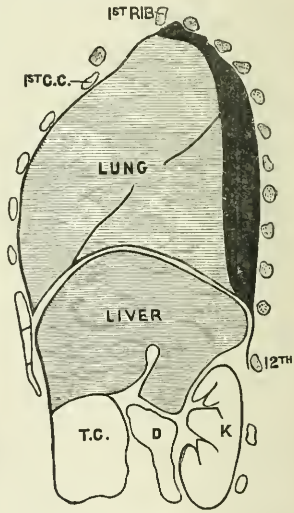


Fig. 1.

As we have already seen, there is a mutual relation between the shape of the liver and the organs lying in relation to it; in fact, His has shown that the liver possesses markings on its surface corresponding to all the organs that lie in contact with it. The liver is not firmly fixed in one position, and it is not perfectly incompressible, so that variations in the size of neighbouring organs may move the liver or alter its shape. The amount of movement, and the moulding of the liver resulting from any given amount of pressure, must depend upon the degree of its fixation and the consistence of its substance.

Braune was of opinion that it is naturally very soft and easily altered in form. Thus he wrote,—"It is open to proof that the form of the liver is not an independent one, but varies with the pressure and volume of neighbouring organs, so that in a normal condition it must possess a softness of structure which can be compared with fat and connective tissue, and which yields to the movements and change of position of the organs in contact with it.[1] Luschka was not inclined to go quite so far as Braune. Thus in his *Lage der Bauchorgane*, p. 25, he stated that "although he was not able to share the opinion of Braune that the liver sub-

[1] *An Atlas of Topographical Anatomy*, by W. Braune. Translated by Edward Bellamy. Text of Plate XV., p. 123.

stance during life is so soft that it possesses no independent form, but is entirely dependent upon the pressure and volume of neighbouring organs, he yet feels entitled to the idea that the complete characteristics of its curves are destroyed by disturbance of its relations. By the removal of the liver from the body it undergoes a distinct broadening out of its fissures, as well as a degree of flattening of its surface, which is far removed from its normal condition."

Landau[1] believes that during life the liver is very soft. He states that in several operations upon the liver for echinococci cysts, he found the liver so soft that he could not distinguish by direct palpation the liver tissue from the cysts.

Of all the organs lying in relation to the liver, the stomach is the one that is subject to the most frequent, marked, and rapid variations in size, and I propose to consider the effects of the changes in the condition of this viscus upon the liver.

Pirogoff[2] and Braune[3] are almost the only investigators who have contributed anything of importance to our knowledge of this question. Pirogoff gives several drawings of frozen sections of the abdomen in cases in which the stomach was empty, and others in which it was distended. Braune, in describing a transverse section of the abdomen (Pl. xiv. of his small Atlas), thus refers to this subject,—"As the space between the liver and the spleen appears to be completely filled up by the stomach, which, however, presents only a slight degree of distension, the question arises, what would be the condition of things if this viscus were distended? It is easily seen that apart from a considerable protrusion of the anterior wall of the abdomen, which is observable after each full meal, the lower ribs also must give way—a circumstance which, under a continued swelling of the abdomen, leads even to permanent prominence of the thoracic segment, as may be proved in many ways, and is especially seen in children. The left lobe of the liver must follow more or less the movements of the stomach since it forms a species of covering to that organ; it is lifted up by the distended stomach, pushing the pericardium up with it, and sinks down with the contracting stomach, the place of which is taken partially by the left flexure of the colon. The mesentery-like left coronary ligament of the liver renders possible such movements of its left lobe, which are associated either with a turning of the entire liver (the axis of which is to be sought in the right lobe corresponding with the strong firm attachments to the right half of the abdomen), or arise from a yielding or distension of the soft tissues."

Neither of these authors give any detailed account of the

[1] *Die Wanderleber und der Hängebauch der Frauen.* Berlin, 1885.

[2] *Anatome Topographica Sectionibus per Corpus Humanum Congelatum.* Petropoli, 1859.

[3] *Topographisch-Anatomischer Atlas*, 1875.

examination of livers with the stomach empty contrasted with those in which the latter organ was distended.

The subject has appeared to me to be worthy of further study, and I have now the honour of presenting to this Society the results of a series of investigations bearing upon this question.

I have no doubt but that valuable information might be obtained by careful clinical observations and by artificial distension of the stomach after death. My own researches, however, have been confined to the comparison of the position and shape of the liver in a number of bodies, in some of which the stomach was empty, while in the others it was more or less distended. In no case was the stomach artificially distended, although it is possible that in some of them gases may have been formed in it after death. The organs were examined by means of frozen sections, the entire body, with the exception of the extremities, being frozen.

The specimens to which I wish, in the first place, to direct your

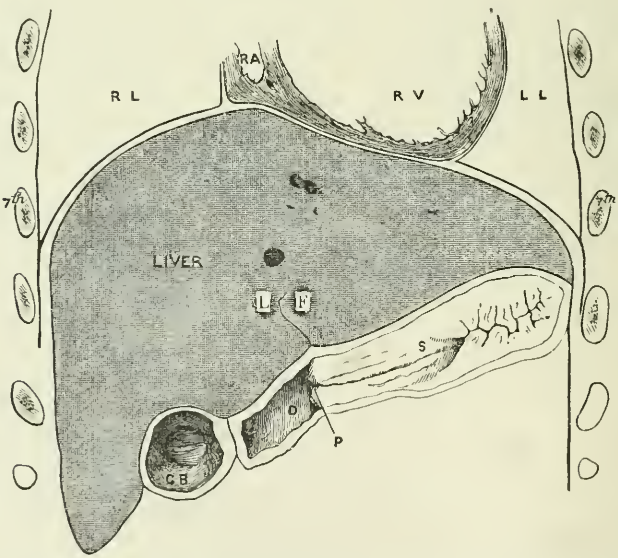


Fig. 2.—Coronal section of part of thorax and abdomen of female child, aged one year and ten months. R L, right lung; L L, left lung; R A, right auricle; L F, longitudinal fissure of liver; R V, left ventricle distended with injection; S, stomach; P, pylorus; D, first part of duodenum; G B, gall bladder, which was full of bile. (Reduced.)

attention were obtained from the bodies of two children. These afford a ready means of comparison, since both were about the same size and were cut up in a similar manner, but in one of them the stomach was empty and contracted, while in the other it was pretty fully distended.

Subject with Stomach empty and contracted.—This was a female child about one year and ten months old. The thorax and abdomen were divided by a series of coronal sections. On an examination of the specimens thus obtained they were found to afford beautiful examples of the appearance of a contracted stomach. Woodcut, Fig. 2, represents a view from the front of one of the sections. In it the stomach was divided where it turns to the right to end in the pylorus. In the coronal section behind this one the stomach was nearly circular, and had a diameter of about an inch. Its peritoneal surface was smooth, but its mucous membrane was thrown into numerous folds, and enclosed a stellate potential cavity. This is partly seen in the woodcut in the left part of the stomach, but it was much better shown in a section about half an inch behind the one represented in this figure. In the portion of the stomach which turned to the right side the folds of the mucous membrane were not so abundant. The pylorus was directed to the right side, and lay close to the mesial plane of the body and immediately below the longitudinal fissure of the liver. As can be seen from the woodcut, the first part of the duodenum passed to the right and somewhat downwards as far as the gall bladder, and then turned backwards. It was in contact with the lobulus quadratus of the liver. The gall bladder was distended with bile. After the sections had been hardened in spirit, the pieces of liver were removed from the different slabs and placed in order, one against the other, so as to build up the liver again. A drawing was then made showing the appearance of the liver as seen from the front (see Plate I.) The notch on the anterior surface between the right and left lobes was close to the mesial plane. Above this notch the line of attachment of the suspensory ligament to the liver passed upwards and somewhat to the right. The connexion of the suspensory ligament to the anterior abdominal wall was almost directly in front of its attachment to the liver, and the ligament itself was thrown into a number of folds. As is shown in the Plate, none of the left inferior surface was seen when the liver was examined from the front, except a small piece on the right side.

Subject with Stomach distended.—This was a female child about fifteen months old. Although younger than the other infant, it was nearly as large. The thorax and abdomen were divided by a series of coronal sections, which nearly corresponded with those made in the other case. Woodcut, Fig. 3, represents an anterior view of one of these sections, which was made at about the same distance from the anterior thoracic wall as the one shown in Fig. 2. On comparing these two figures, a considerable difference may be noticed in the position of both the liver and the stomach. The distension of the stomach, as seen in Fig. 3, has evidently been associated with a movement of the pylorus towards the right side, and a twisting of the pyloric part so that its orifice is directed backwards instead of to the right. Then the left lobe of the liver has moved over towards the right side, so that the left border no longer lies in contact with the left lateral wall of the abdomen, and, indeed, scarcely reaches so far as the left border of the heart. The anterior part of the longitudinal fissure of the liver which, in the case with the stomach empty, was close to the mesial plane, is here distinctly to its right side.

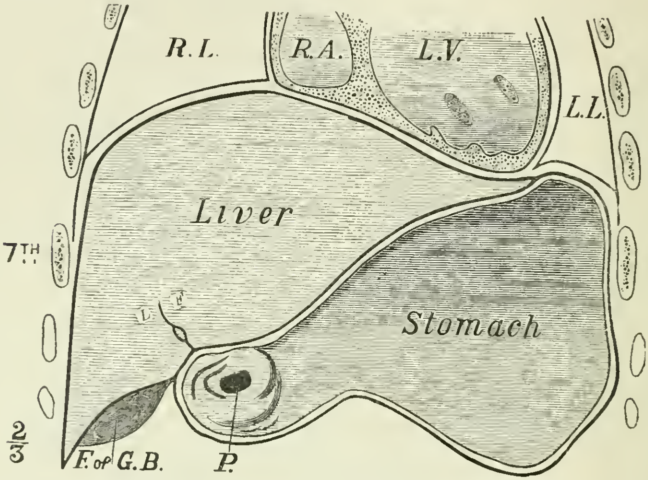


FIG. 3.—Coronal section of part of thorax and abdomen of female child, aged one year and three months. R L, right lung; L L, left lung; R A, right auricle; L V, left ventricle; P, pylorus; L F, longitudinal fissure of liver. (Reduced.)

When the sections were hardened in spirit, the pieces of liver were removed from the different slabs, and after they were put together, a drawing was made in the same manner as in the former case. Plate II. shows an anterior view of the liver thus reconstructed. This Plate, when contrasted with Plate I., illustrates even more distinctly than the Woodcuts, Figs. 2 and 3, the changes in the position and shape of the liver caused by a distension of the stomach. The dotted line indicates the middle line of the body. The greater part of the left lobe lies to the right of the mesial plane, and the fissure on the anterior part of the lower surface, separating the right and left lobes, is nearly $1\frac{1}{2}$ inches from the middle line. Above this fissure the line of attachment of the suspensory ligament to the liver is directed upwards and to the left, whereas in the specimen with the stomach empty it inclined upwards and to the right. This indicates that in the gliding of the anterior surface of the liver towards the right side the lower part moved much more than the upper. The anterior surface of the right lobe was probably compressed, and also rotated round to the right side. It is evident from the drawing that the anterior part of the under surface of the liver was pushed up by the distended stomach.

These two cases support the view that the pressing of the liver over towards the right side, and consequent diminution in its transverse diameter, is associated with an increase in the vertical and antero-posterior diameters of the right lobe. Thus the diameters of the liver in the case with the stomach empty were as follows:—Transverse $4\frac{1}{4}$ inches, vertical $3\frac{1}{4}$, antero-posterior $2\frac{1}{2}$. In the other liver the transverse diameter was scarcely $3\frac{3}{4}$ inches, but the vertical and transverse diameters were $3\frac{3}{4}$ and $2\frac{3}{4}$ inches respectively. The differences in the position and shape of the liver in these two cases are such as we might expect to result from distension of the stomach, but alone they are not sufficient evidence of its normal occurrence. I possess, however, a number of other specimens, all of which confirm the opinion derived from the examination of those two cases. Thus Woodcut Fig. 4 was prepared from a horizontal section of the abdomen of a child in which the stomach was empty. The liver is seen to reach to the left side between the spleen and the diaphragm, and the attachment of the suspensory ligament to the liver to be opposite its connexions with the abdominal wall, so that the ligament was lax and folded.

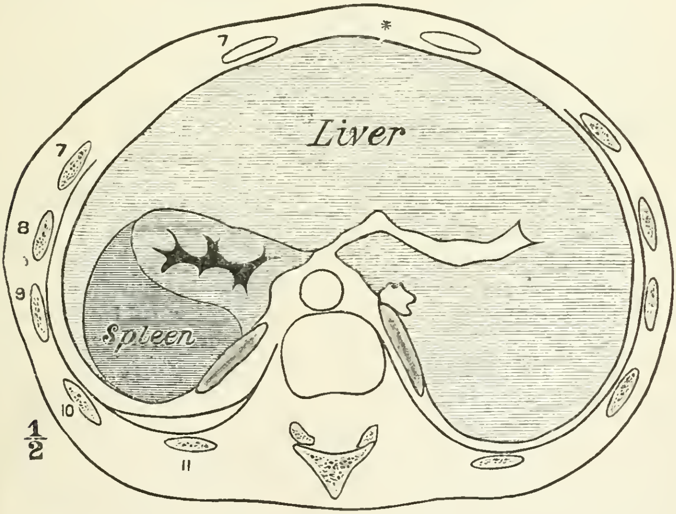


Fig. 4.—Horizontal section of trunk of a boy, 5 years old, at level of disc between 11th and 12th dorsal vertebræ.
* In front of suspensory ligament of the liver.

In contrast with this, I show you a horizontal section of another child in whom the stomach was distended (see Woodcut, Fig. 5). Here the suspensory ligament was tense, and its surfaces were parallel with the anterior abdominal wall. Its origin from the wall of the abdomen was about $1\frac{1}{2}$ inches from its attachment to the liver. The part of the left lobe of the liver divided in this section lay almost entirely to the right of the mesial plane. Above the plane of this section the left lobe reached to the left side as far as the dotted line in the woodcut.

I have several other specimens in which the stomach was distended, showing a similar condition of the suspensory ligament.

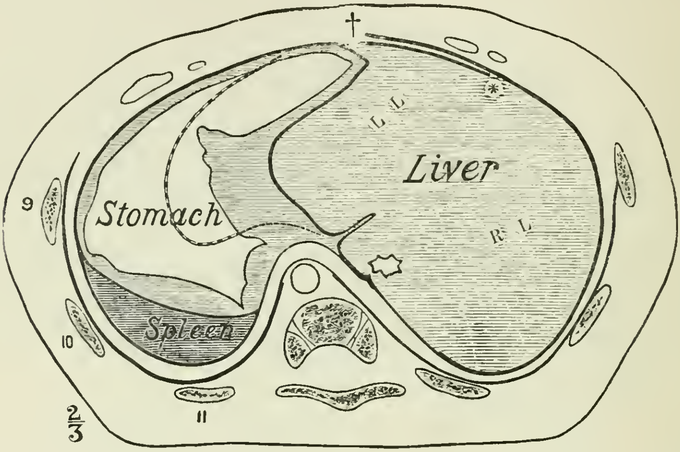


FIG. 5.—Horizontal section of the body of a female child, aged 12 months, at level of 11th dorsal vertebra.
† Attachment of suspensory ligament to abdominal wall. * To liver.
L L, left lobe. R L, right lobe.

The specimens I have hitherto described were obtained from the bodies of children, but I will now direct your attention to the case of a man, aged 57 years, who died of cancer of the pharynx. He was much emaciated, but his thoracic and abdominal viscera were found to be healthy. The entire body, with the exception of the lower extremities, as in the other cases, was frozen, and the part below the level of the mouth divided by horizontal sections into a series of slabs, which on an average were about an inch in thickness. These specimens were traced, embedded and hardened in spirit in the usual way. On examination, it was found that both the stomach and splenic flexure of the colon were distended, and that the position and shape of the liver were very similar to those of the children with distended stomachs. Plates III. and IV. show the shape of the liver as seen from the front and from behind, and its relations to the body wall. These Plates were constructed from the transverse sections by a plan similar to that followed in the preparation of Plate XII. of my work on the *Anatomy of the Child*, viz., a drawing was made of the body before it was frozen, and when the sections were made, their position was indicated on the drawing by transverse lines. The parts of the skeleton and viscera divided in the plane of each section were marked upon the drawing, which was completed by the dissection of the slabs.

Plate III. shows an anterior view of the thorax and abdomen from the level of the 7th dorsal vertebra to the disc between the 4th and 5th lumbar vertebræ. The numbers 12 to 24 indicate the position of the sections, which were made between these two planes. As already mentioned, the patient was very thin, and the anterior abdominal wall was so much retracted, that it lay close to the bodies of the lower lumbar vertebræ. The ribs were more oblique, and the lower true cartilages ascended in a more vertical direction than normal. This was probably due to the falling inwards of the lower costal arches in consequence of the retraction of the abdomen. The highest part of the upper surface of the liver was just below the level of the 4th rib, at its junction with its cartilage. The upper surface, when traced towards the left, was found gradually to descend, and to end in the left border one and a half inches to the left of the mesial plane. Behind the ensiform process and the 6th and 7th costal cartilages, it was overlapped by the right ventricle of the heart. The lower edge of the right lobe was at the level of the tip of the 11th costal cartilage, and only about an inch above that of the umbilicus. This is certainly lower than normal. It was evident, from the position of the longitudinal fissure and the line of attachment of the suspensory ligament to the liver, that the anterior surface of the liver had glided over towards the right side; thus, in the section marked 20, the longitudinal fissure of the liver was $2\frac{1}{2}$ inches to the right of the mesial plane. Only a very small surface of the liver was in contact with the abdominal wall below the ensiform cartilage. It is generally estimated that the liver lies behind the abdominal wall in the upper third of the space between the ensiform cartilage and the umbilicus. Here it only extended downwards about half an inch below the tip of the cartilage.

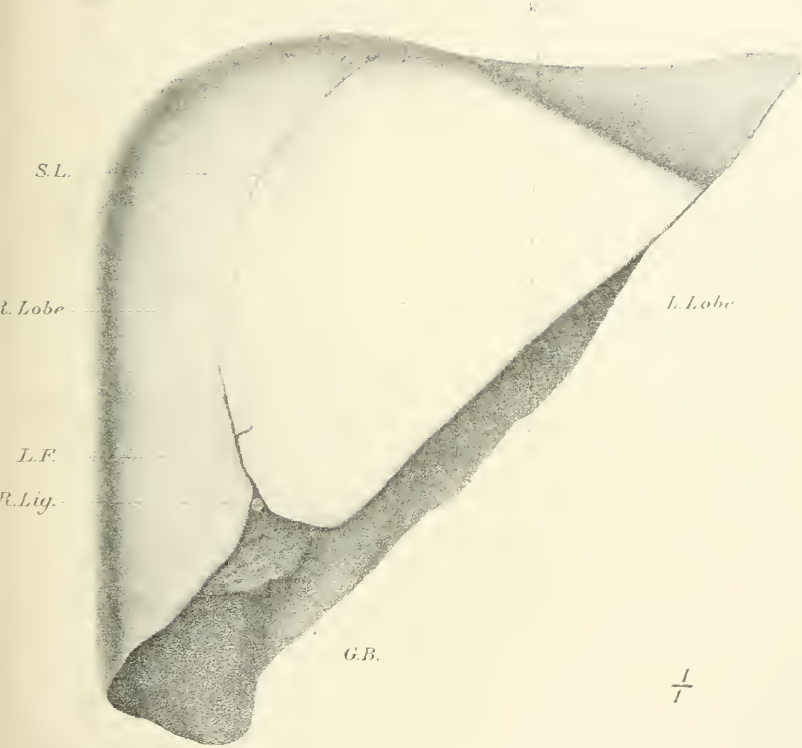
The gall bladder was full of bile, and its fundus was situated below the longitudinal fissure of the liver, and just behind the 8th costal cartilage close to its junction with the rib. The gall bladder is usually supposed to be placed close to the tip of the 9th costal cartilage, while in this case it was two inches to the right of that point. The gall bladder must move with the liver, but in this subject its fundus was rather long and movable, and projected forwards below the anterior part of the longitudinal fissure instead of being to the right of the fissure.

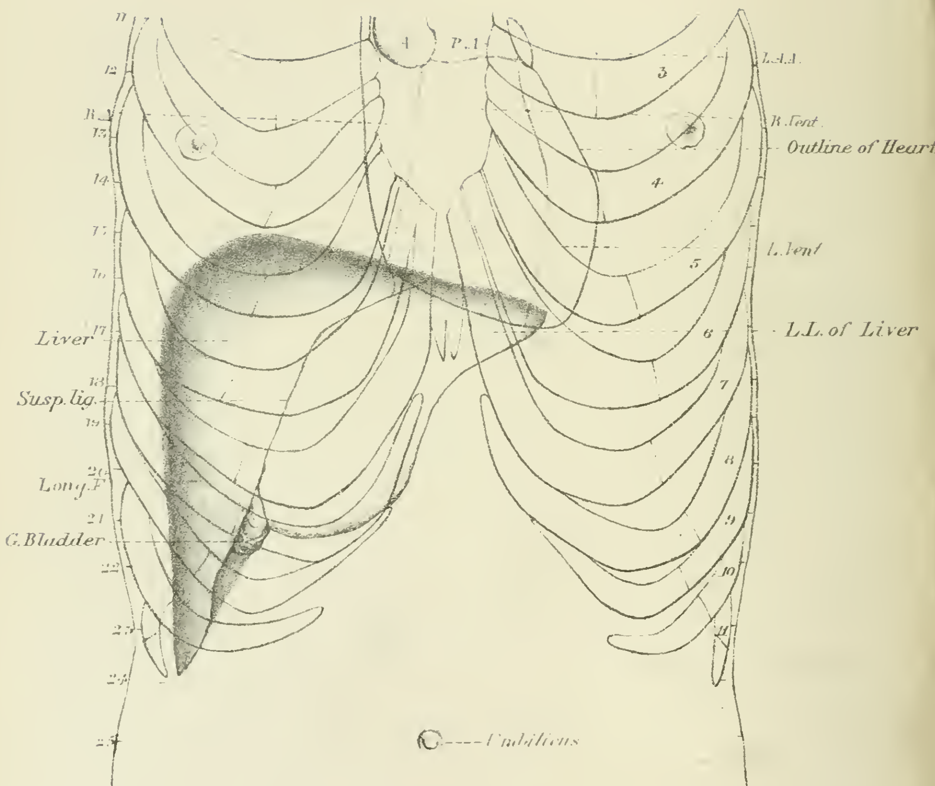
Plate IV. represents a view from behind of the thorax and abdomen from the level of the body of the 8th dorsal vertebra to the upper part of the sacrum. Most of the illustrations in our text-books, showing the abdominal viscera from behind, are evidently based upon plate ii. in Luschka's *Lage der Bauchorgane.* This plate contains some very obvious errors, so that it is of but little use for purposes of comparison. Thus the transverse process of the 5th lumbar vertebra is represented as being higher than the top of the iliac crests, the kidneys are placed entirely external to the transverse processes of the lumbar vertebræ, and the right kidney is made to extend downwards to a point opposite the transverse process of the 4th lumbar vertebra. I regret that I have not at present any sections of adults with the stomach empty with which this series might be compared. The inferior vena cava and also the large hepatic veins were distended with blood. The inferior vena cava and the Spigelian lobe of the liver were in their normal positions. The renal impression of the liver was of large size, and lay in close contact with the greater half of the antero-external surface of the kidney, while the lower border of the liver reached fully an inch below that of the kidney.

General Conclusions.—The various specimens that have just been described show that the distension of the stomach causes certain definite alterations in the position and shape of the liver. The movement of the liver is a somewhat complicated one, but may be roughly described as a rotation round a vertical axis passing through the inferior vena cava. The portions of the liver that undergo the greatest displacement are the left lobe and the anterior part of the right lobe. It is interesting to note, in connexion with this, how the relations of the peritoneum facilitate this movement. Thus the whole of the anterior surface of the liver and the corresponding part of the abdominal wall are covered by peritoneum, while the left lateral and suspensory ligaments of the liver are long mesentery-like folds. There has been much dispute with reference to the function of the so-called suspensory ligament, but I believe that the preparations I have shown afford a very evident explanation of its use. When the stomach is empty the ligament is relaxed and thrown into a number of folds; as the stomach fills, the anterior surface of the liver glides over towards the right side, and the ligament ultimately becomes tense, and thus tends to check excessive movements of the liver. The round ligament and the left lateral ligament are associated with the suspensory ligament in confining the movements of the liver to within a certain range. In the fœtus the stomach is empty or nearly so, and it is fortunate that this is the case, for a dilatation by pushing over the liver to the right side and consequent bending of the umbilical vein would be very apt to obstruct the supply of arterial blood to the fœtus.

While the left lobe and the anterior part of the right lobe are very mobile, certain other portions of the liver undergo little or no change in consequence of distension of the stomach. Thus I have not been able to obtain evidence of any change in the position or direction of the inferior vena cava. With reference to the Spigelian lobe, if there be any change in its position it is a very slight one. The surface of the lobe lies in contact with the

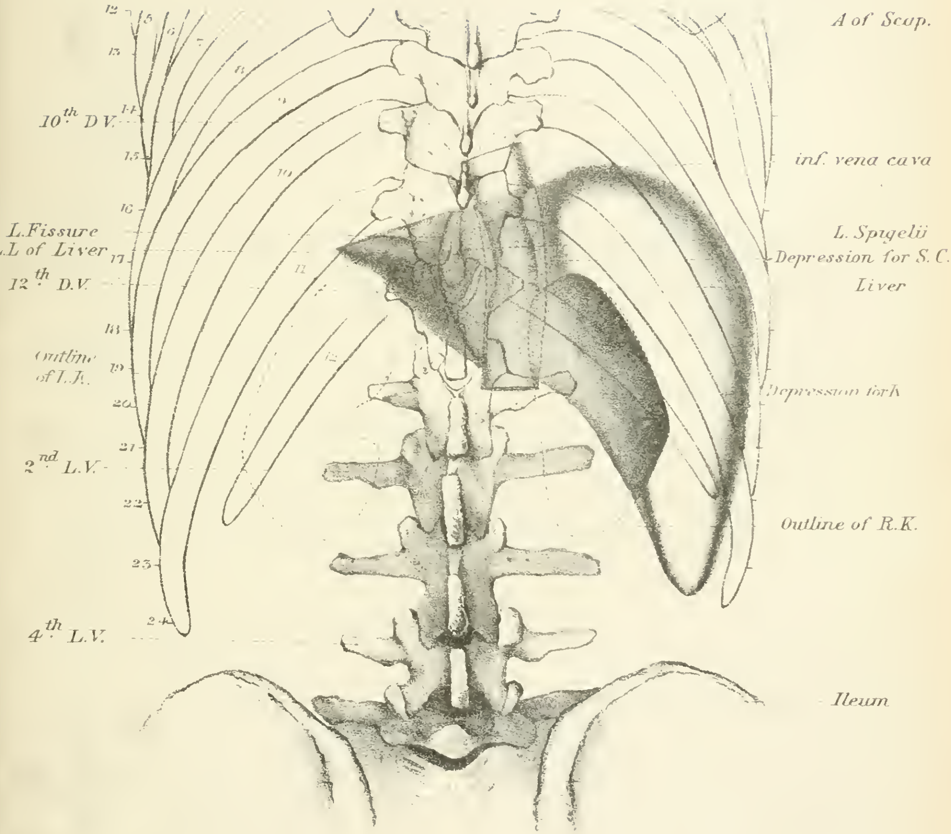
PLATE I.


S.L.
R. Lobe
L.F.
R.Lig.
L.Lobe
G.B.
$\frac{1}{1}$


Liver
Susp. lig.
Long. F
G. Bladder
Outline of Heart
L. L. of Liver
Umbilicus

$\frac{1}{3}$

PLATE IV.



$\frac{1}{3}$

diaphragm (except that both are covered by peritoneum) and looks backwards and inwards. It is possible that it looks a little more inwards when the stomach is distended. Then the posterior surface of the liver, lying to the right of the inferior vena cava and above the kidney, being uncovered by peritoneum and united directly to the diaphragm by rather firm connective tissue, cannot move to any appreciable extent. The shortness of the coronary ligaments also implies the absence of a movement of this part of the liver. In the man, aged 57, the liver was found to extend slightly inwards behind the kidney. This may have been due to the compression of the liver in the right hypochondrium. It is probable that the gliding of the anterior surface of the liver towards the right side is associated with a corresponding backward movement of the right surface. There are, however, no markings on this surface to assist us in determining whether or not this is the case.

Whether the stomach be empty or full, the liver always retains its general form, which is that of a triangular prism. The transverse diameter is, however, considerably diminished, and the vertical extent on the right side increased by a distension of the stomach. The two specimens in Plates I. and II. support this view, which is rendered still more probable from the appearance of the liver in the man aged 57 years. It will be remembered that in his case the liver was not at all enlarged, yet the right lobe reached on the right side lower than the kidney, a condition which is certainly not regarded as normal. This increase in a vertical direction is what one would naturally expect from the compression of a plastic mass like the liver placed between a distended stomach and the right lateral wall of the abdomen.

Explanation of Plates.

Plate I.—Anterior aspect of liver of female child, aged one year and ten months, with stomach empty and contracted.

M L, Middle line of body. *L F*, Longitudinal fissure. *S L*, Suspensory ligament. *R L*, Round ligament. *R Lobe*, Right lobe of liver. *L Lobe*, Left lobe of liver. *G B*, Gall bladder.

Plate II.—Anterior aspect of liver of female child, aged one year and three months, with stomach distended. Letters same as in Plate I.

Plate III.—Anterior aspect of body of man, aged 57 years, from level of the 7th dorsal vertebra to the umbilicus. The stomach and splenic flexure of colon were distended.

Plate IV.—Posterior aspect of body of same subject as that shown in Plate III.

Professor Grainger Stewart considered the paper was very instructive in respect of a number of clinical points. For example, it threw light on a point in regard to which he had often felt himself in difficulty. A mass might be felt pretty far to the right in the abdomen, and the question arise as to whether it was the gall-bladder or a tumour, perhaps cancerous in character. Hitherto in such cases the diagnosis of gall-bladder was often set aside, because the mass seemed too far from the middle line to be that viscus; but the observations of Dr Symington showed that rotation of the liver took place under the influence of a distension of the stomach and other hollow viscera, and explained the position of the gall-bladder in such circumstances. This matter of rotation of the liver was also of importance in relation to changes in parts of the liver itself. Suppose an enlargement of the left lobe from the presence of a syphilitic mass or a cancerous tumour, they could understand how there might be both an alteration in the form and some degree of rotation from the presence of such mass. He could recall cases of obscure tumour in the back part of the abdomen in which these observations might have been of great assistance.

Dr James asked if Dr Symington had taken into account the effect of respiration on these variations in the position of the liver. Clinically he had been struck with one fact, that the extent of the lower border of the liver did not correspond with the extent of the epigastrium. In inspiration a projection of the epigastrium was observed. This projection often seemed to occur to a greater extent than displacement of the liver downwards would seem able to account for. Dr Symington's observations suggest an explanation of this. In inspiration one might suppose that the liver would be pushed down *en masse*, but it was more that the lung descended and got between the liver and the anterior abdominal wall. In connexion with the effect of respiration on the position of the liver, he remembered a plate in Sibson's work showing the anterior border of the liver half-way down to the umbilicus. In this case the patient was killed suddenly. He wished to ask if Dr Symington knew whether any observations had been made as regards the effect of the condition of the lungs on the position of the liver?

Dr Symington, replying, said that transverse vertical sections showed that in healthy conditions the right vault of the diaphragm was always higher than the left, even when the stomach was distended. In such cases he thought he would find the left vault higher, but in the dead body he had never done so. He thought this was an additional proof that when they had a distended stomach pushing the liver over to the right side there was an equal projection upwards of the right vault with the left. As to the effect of respiration he omitted all reference to it, because it was not a subject which could be studied on the dead body. He admitted that there might be some truth in the observations of Dr James; but his feeling was that, if the liver did not descend so much as it should, it was because the antero-posterior diameter would be increased, and the liver flattened more from above downwards.

2. THE TREATMENT OF FIBROID TUMOURS OF THE UTERUS BY ELECTRICITY.

By Skene Keith, M.B., F.R.C.S. Ed.

Gentlemen,—As it now seems to be within the range of possibility that operations for the removal of fibroid tumours of the uterus or for checking their growth and making them of little or no account, may have to be abandoned for a safer means of treatment, you may think that some apology is required for bringing a subject, therefore, belonging entirely to gynæcology before you, more especially as the subjects themselves expect to be safe from the knife of the surgeon, and have already tried what medicine can do for them, without any very gratifying results.

For a number of years we have had amongst us the authority on the treatment of vascular tumours by electricity; and, as far as I am aware, we have been content to take credit from what he has done, without making any effort ourselves to make new use of this great and unknown force. Even when experiments are being made in the treatment of different diseases in different parts of the world, we appear to be anxious to wait and see what others are doing before trying anything for ourselves. This conservatism would be perhaps highly commendable if, when proper precautions were taken, and these have been worked out by Dr Apostoli for all general surgical purposes, there was much danger to life. One certainly prefers that if a death be inevitable some one else should have to do with it than ourselves; but there does not appear to be much risk in the application of electricity to several purely surgical diseases. In this matter we are behind instead of in front of the times. In Glasgow, the treatment of stricture of the urethra by this means is an old story, and enlargements of the prostate have been attacked by the same agent. It has also been used successfully in the treatment of lupus, and moles can be removed from the face without leaving a cicatrix, though this belongs rather to decorative than to legitimate surgery.

I do not ask every one who has a patient suffering from stricture of the urethra to attempt its cure by means of electricity, because in that case there would be many disasters. What is wanted is a series of cases, and in patients whose record can be followed for several years. I do not believe that there is any more danger in applying electricity to a stricture than there is in passing a bougie —a bungler will do perhaps as much harm one way as the other. I would say, give the treatment a fair chance, and then tell us the result. It is astonishing to notice how, in the treatment of fibroid tumours of the uterus, Dr Apostoli's directions have been simply ignored by many in the great anxiety to make something to which to attach one's name. For example, we have improvements on Dr

Steavenson's electrodes by those who are treating cases after what they suppose to be Dr Apostoli's method. We were told in Dublin that it was necessary above all to have a good knowledge of pelvic diagnosis; yet flexible electrodes are advocated because they are more easy of introduction than rigid ones, and can be got into the cavity of the uterus when a rigid one cannot. What is this but an acknowledgment of the want of the first great essential—a knowledge of pelvic diagnosis and manipulation?

The apparatus suitable for the treatment of uterine fibroids is all that is requisite for the treatment of surgical diseases, except, of course, the special electrodes necessary for each disease. It will not probably be necessary to have so powerful a battery, as weaker currents appear to be sufficient for most conditions for which electricity has yet been tried. Taking the maximum body resistance, including that of the pad, screws, wires, etc., at 500 ohms, and the current which is required at 30 milliampères, we find that ten or twelve large Leclomaché cells would prove sufficient. This resistance I have found is far above the average, and in this I am corroborated by Dr Morrice of St Bartholomew's Hospital, who estimates it at from 150 to 300 ohms, and gives an example where it was considerably less than the smaller number.

One disease for which it is to be hoped this treatment will prove effectual is that of enlargement of the prostate, a disease coming on usually at an age when one expects that the hardest part of the life struggle has ended, to dash all hopes of a comfortable old age. It is not pleasant to look forward to the possibility of carrying a catheter in one pocket and a wide-mouthed bottle in the other.

I have wandered off my subject, and shall now briefly narrate the histories of a few of the cases of fibroid tumours of the uterus treated by Dr Keith and myself by electricity, selecting those where there has been a decided decrease in the bulk of the growth, as it is only those which bear on the diseased conditions already mentioned. I can only vouch for the primary results, but there is enough to encourage one to try this treatment in other conditions, and if it does no good it need certainly do no harm.

Case I.—Miss H., age 47, remembered that she had had a tumour for more than twelve years. The mass was a great burden to her. She suffered from severe bleedings and from retention of urine almost every month. The tumour was large, extending far up beneath the ribs, and must have weighed nearly 30 lb. It filled the whole of the pelvis, and the cervix could not be reached, as there was no room to pass the finger between the pelvic portion and the pubes. The bladder was situated in the abdomen, and the urethra measured rather more than five inches. She had little faith in treatment by electricity, but was willing to have it tried.

On 11th July the tumour was punctured from the vagina by the needle attached to the negative pole, and a current of nearly 200 milliampères passed through it for five minutes. In the middle of August, after the fifth sitting, the patient thought that she could walk more easily, and the finger could be passed up to the cervix. There had been as much trouble as usual at the period. After another sitting the tumour was much more moveable, and was noticed to be distinctly nodular. Menstruation came on after the eighth application without pain or even discomfort, the first time for years. There was less discharge and no bladder trouble. It was not until after the thirteenth sitting that the tumour could be felt free of the ribs, and it was judged that it must have lost 10 or 12 lbs. in weight. The treatment is not yet finished, and has been much prolonged, as the patient has to attend to her shop, and is thus on her feet all day, and it has not been always easy for her to get some one to look after the business in her absence. She leaves her work, has the treatment, and goes back again, and has never been laid up even for an hour. Her waist is not yet an elegant one, but is not now situated about her armpits as it used to be.

Case II.—Mrs C., age about 36, from Dr Wemyss, Broughty-Ferry, had about two years ago a vomiting of blood at the time when she should have been unwell, and apparently taking its place, and there was no flow from the vagina that month. From that time she has suffered from constant pelvic pains and pains in the left leg, from watery discharge, and from diminished menstrual flow. The uterus was large, lay forwards, and the cavity measured $4\frac{1}{4}$ inches. Behind and filling almost the whole of the pelvis there was a hard mass, evidently a fibroid. Operation: removal of the ovaries had been agreed to, and would have been performed if electricity had not been tried. The treatment consisted in fourteen negative applications and two negative punctures, and extended over a period of five weeks. The result was that the uterine cavity became reduced to $3\frac{1}{2}$ inches, the mass, which previously had about filled the pelvis, felt about the size of a somewhat enlarged ovary, and the lady went home free from pain and feeling perfectly well.

Case III.—Mrs B., age 57, brought by Dr M'Gibbon, had seen Dr Keith thirteen years ago with a fibroid tumour which did not extend as high up as the umbilicus. Although menstruation ceased a number of years ago, the tumour continued to grow until it reached under the ribs on the right side, and was quite immovable. The old lady suffered great discomfort from the size, from great pain in the right side and thigh, and from very great bladder irritation. The tumour was very hard, filled the pelvis, and was protected from puncture, however, by the bladder. The sound was therefore passed into the cavity and attached to the negative pole. After the fourth sitting, the patient said that she felt as if she had taken a new lease of life, for she had expected that her days would soon have been numbered. After nine sittings, she said that she would be quite content with the improvement which had been gained.

The twenty-third and last application was made on the 20th October, and at that time the tumour was not half the size it had been. Since then the patient got a chill when crossing the Burntisland ferry, and when heard of the tumour had become decidedly swollen, although the bladder trouble had not come back.

Case IV.—Miss G., age 50, from Dr M'Kercher, Dalbeattie, had been tormented by her tumour for twenty years. She scarcely remembers having a good night's rest, is constantly in pain, and for the last five or six years has spent at least six months out of every twelve in bed. The patient could scarcely walk, and had to be assisted into the room by her servant. Examination of the abdomen had to be made with the greatest care on account of excessive tenderness. The tumour was markedly irregular, extended on the right side up to the level of the umbilicus; on the left it was higher, and there was a mass filling the whole of the left loin. There were 23 applications, of five minutes each, made of from 100 to 200 milliampères. After three sittings, the tumour could be handled freely; after seven, it was decidedly smaller, and she said that she thought she would have been dead if her doctor had not sent her up to town. Five weeks after the commencement of the treatment, she walked fully three miles, and two weeks later went home. From a delicate, miserable creature she had been transformed into a fine, handsome woman. The tumour did not extend more than two inches above the pubes on the right. The mass which had been in the loin did not reach outwards as far as the anterior iliac spine, and was not higher than the umbilicus, and, moreover, the cervix was almost out of reach.

Case V.—Miss G., age 40, had first had her attention drawn to the presence of her tumour sixteen months ago. At that time she lost a great deal of blood every month, but a long rest in the country had greatly improved her general condition, and she looks fat and well. The tumour was not a large one, extending only up to the umbilicus. The treatment has consisted, as in the previous cases, of negative applications to the interior of the uterus. After five of these she felt that her jacket was loose, and that she could go upstairs without resting and panting Now, after seventeen sittings, the tumour scarcely reaches to within two inches of the umbilicus, and the patient feels very well.

Case VI.—Miss W., age 44, from Glasgow, had been told by

Dr Keith four years ago that she had a fibroid tumour. In the interval the growth has increased considerably. She has suffered from profuse menstruation for eight or ten years. The tumour extended to one inch above the umbilicus on both sides, and the cavity measured $6\frac{1}{2}$ inches. Seven positive applications were made to the interior of the womb, but as menstruation came on more profusely than usual, thirteen negatives have been since made, so as to reduce the size as quickly as possible. The second period has been scarcely so bad as the first. Now there is a marked difference in the size and shape of the tumour. On the right side it has become lobulated, and is as high as the level of the umbilicus; on the left it has almost disappeared, and it is interesting to note that this part of the tumour had grown quite lately.

What has become of those tumours, or how they have partly disappeared, I do not know. At first one somewhat naturally supposed that the softer the tumour was the more quickly it would become reduced in size; but we have found that this is not so, and that it is the hard ones whose bulk is most easily reduced. It remains a fact that they do disappear to a certain extent, and this result is obtained without much risk, without much pain, and without confining any one of them to bed or even to the house.

Dr John Duncan said the subject which Mr Keith had so practically considered was one which was of considerable interest to him, as for more than twenty-five years he had been working electrically for the cure of various forms of growth. Long ago, when electrolysis was introduced as a treatment for aneurism, he ventured to extend it to a number of other forms of tumour, and, after experimenting, came to the conclusion that it was admirably suited for certain forms of nævus, by far the best form of treatment for cirsoid aneurism, and might be extended to other diseases, such as vascular goitres. The further extension of the treatment to which Mr Keith had drawn attention did not, of course, fall under his observation, and on the particular subject of uterine fibroids he could say nothing. Still, as analogous to what he had seen in other instances, it was a matter to him of much interest. The chief point of interest was as to the method whereby the currents of electricity produced those remarkable results to which Mr Keith had drawn their attention. So far as he could make out from Mr Keith's paper and from Apostoli's writings, it appeared not to be essential that the tumour should be punctured. That was a matter of great importance. He had tried the effect of tolerably strong currents passed through various forms of tumour, enlargement of glands, goitres, etc., without breach of surface. He tried with smaller cells certainly, but was disappointed with the effect for discutient purposes. Electric currents had a most distinct effect on nerves and on muscles, and doubtless also on all mobile elements in the body, but in so far as these effects might be used for the discussion of tumours he was disappointed with them, gave them up, and restricted himself to purely electrolytic effects, which consisted simply in the decomposition of so much tissue, the negative pole corresponding to the action of alkalies, the positive to that of caustic acids. This electrolytic action he also tried on various morbidities, and he had not the slightest doubt that they could cause an innocent tumour to disappear by electrolysis; but it would take a very long time to do so, a number of operations, and would produce as much pain and discomfort as the knife, and that therefore it was better to cut them out. In the case of vascular tumours, on the other hand, by electrolysing extensively and chiefly with the negative pole they could advantageously bring about a discussion of the tumour. That such an operation might be applied to uterine fibroids with advantage he believed to be extremely probable. That it was useful in other forms of tumour there could be no possible doubt, and he believed that if it were easy to pass it into the prostate gland it might be of the use Mr Keith had indicated. It might be used in enlarged tonsils, but cutting was equally effectual, and quicker. As to stricture, the action was purely electrolytic produced by the negative pole, by applying the pole to the upper end of the stricture and forcing it through, causing a destruction of tissue. This was in some respects similar to the alkaline caustic treatment, and *à priori* one would suppose such destruction of tissue to be contrary to sound views of treatment, and that the gradual dilatation by bougies was safer. A more immediate effect was certainly obtained, but the risk and the probability of reproduction he thought was considerable. There was a third method in which the current of electricity might act on these fibroid tumours. This was the application of the electrolytic action as a counter-irritant on the inner surface of the uterus. This he believed to be very unlikely. He could hardly conceive that any electrolytic action on the inner surface of the uterus could have such an effect on the tumour. Therefore in Apostoli's treatment it must be either the neurotic influence causing trophic changes, or the electrolytic and destructive. Mr Keith said the application to the uterine wall was as effective as the introduction of the pole into the tumour. The electrolytic action was, therefore, not the method employed. It must, therefore, be the neurotic, and this was a very remarkable conclusion to come to. It was medical and not surgical electricity, a very strong current acting medically and causing discussion. There was no advantage, then, in producing the little electrolytic action that could be produced by five minutes within the uterus. The object of the operator ought to be to apply the currents without producing any electrolytic action locally. This might be done by broadening the internal electrode.

He had, therefore, a very considerable belief that the distinction Apostoli had been making between the positive and negative pole was not real in the case of these tumours.

Dr Foulis asked if faradic or interrupted currents which caused a contraction of muscular fibres, and were useful in cases of post-partum hæmorrhage, had been tried in the treatment of these tumours. Mr Keith said he used the negative insertion in one case to rapidly reduce the size of the tumour. Was the action in that case electrolytic?

Dr Keith, in reply, said he was much interested in all that Dr Duncan had said, and agreed with a great deal of it. Naturally, at present he was inclined to accept Apostoli's statements. In one case, where they had used the positive pole on every occasion, the tumour had decreased as satisfactorily as any of the others. Apostoli, he observed, was getting fonder of punctures.

Dr Duncan.—Yes, and so adding the electrolytic effect to the purely neurotic.

Dr Keith thought the advantage of the treatment as regards urethral stricture was that the results were permanent, and did away with the necessity of passing bougies afterwards.

Dr Duncan remarked that Syme, Holt, and others said the same of their special modes of treatment.

Dr Keith further said he had treated one case of enlarged thyroid in this way, putting two bits of clay on each side of the neck. They thought the gland was diminished one-half, but it remained to be seen whether the diminution was permanent. Dr Foulis's reference to post-partum hæmorrhage did not hold in the cases of tumours. The results of the faradic current had not been permanent. One case Dr Keith had seen in Paris well illustrated the value of Apostoli's treatment. It was the case of a woman aged 43, who three years before had been treated for a large fibroid with menorrhagia. She had not been touched since she was first under treatment, and was, and had remained, practically well.

Meeting III.—January 18, 1888.

Dr John Smith, *President, in the Chair.*

I. Election of New Members.

The following gentlemen were elected Ordinary Members of the Society:—James Lockhart Wilson, M.B., C.M.; William Booth, F.R.C.S.E.

II. Exhibition of Patients.

1. *Dr Byrom Bramwell* showed a case of OPTIC ATROPHY PRECEDING LOCOMOTOR ATAXIA.

2. *Dr Affleck* showed a female patient who had recovered from PERNICIOUS ANÆMIA. She was admitted to his ward in November 1887, suffering from a degree of anæmia which it was impossible for him to convey any idea of. She was in a state of extreme prostration, almost moribund, and did not appear as if she could live more than a week. There was no visceral organic disease to account for her condition, but on examining her blood it was found to present the characteristic appearances of pernicious anæmia. The white corpuscles were not reduced in number, but the red corpuscles were not more than 790,000 to the cubic millimetre. They were also altered in character. Some were large, some small, and they presented all manner of shapes. The hæmoglobin was also diminished. The other symptoms coincided. There were no retinal hæmorrhages, but there was profuse epistaxis, persistent pains, and hæmatemesis. She was placed on a diet of peptonized foods, because it was obvious she had no digestive power for ordinary foods. She had then iron and arsenic and nervine tonics, such as strychnine. The blood corpuscles and hæmoglobin began to increase, and when last examined there were 3,300,000 red corpuscles per cubic millimetre. The case was interesting as indicating a call for a change in the terminology of this disease. It was the second within two years which had recovered in his ward. Dr Pye Smith had recorded similar recoveries.

III. Exhibition of Specimens.

1. *Dr Byrom Bramwell* showed—(*a.*) A CAST OF A CASE OF THORACIC ANEURISM. (*b.*) PHOTOGRAPHS OF THE BRAIN in a case of cancer of the cerebellum and lenticular nucleus.

2. *Dr John Playfair* showed a PIN impacted in the left bronchus. The specimen consisted of the lungs and trachea of a little girl, aged about 15 months. The pin, which was one with a beaded head, was swallowed on 4th May 1886. She was taken first to the Leith Hospital, then to the Royal Infirmary, and on 6th May, two days after the occurrence, to the Sick Children's Hospital. She suffered greatly from dyspnœa, and appeared to be sinking. The house-surgeon, Dr Dewar, at once performed tracheotomy. The breathing became easier, and she rallied. She remained a good many months in Hospital, and a week or ten days before she left it was observed that there was a slight dulness over the left

lung and deficient expansion. Dr Dewar asked the parents to let him know if ever she became ill, and he was called to see her one day in December last. She was dead—of cancrum oris—before he saw her. He obtained leave to make a post-mortem examination, and found the pin impacted in the position stated. The tracheal cicatrix was perfectly healthy.

3. *Mr A. G. Miller* showed a KNEE-JOINT amputated for syphilitic disease, and read the following notes of the case:—A. D., æt. 21, 1st Scots Guards, admitted 23rd November 1887. *History.* —Family healthy. Contracted syphilis February 1886. Treated in Dublin and Woolwich Hospitals, where he had mercury. Knee affected in February 1887. First mercurial treatment continued till June 1887. Went home to Burntisland September 1887. *Condition.*—Great emaciation, enlarged liver, albuminuria, diarrhœa, mesenteric glands tender; vomits every kind of food; has been getting nothing but brandy. *Locally.*—Cario-necrosis of frontal bone; specific ulcers on right side of face and neck following gummata; cario-necrosis of sternum in two places. Gummatous sores on chest and legs. Necrosis of left fibula. Marks of old gummata and ulcers on various parts of the body. Right knee swollen, flexed, and painful. Fluctuation from fluid in joint. *Treatment.*—Valentine's beef juice. Potassium iodide, gr. xx. doses. No brandy. Iodoform locally. Patient began to improve at once. Diarrhœa stopped; albuminuria less; liver smaller. In beginning of January 1888 all wounds were healed except two on sternum. Knee worse. Sequestra removed 20th December 1887. On 6th January 1888 amputation of leg above knee-joint was performed. Patient has since done well, and stump is almost healed. Examination of knee-joint after removal and injection by Mr Caird. Gumma in tract of tibia occult. Evidence of a gumma in crest of tibia, which had burst into joint, and caused disorganization (just at posterior crucial ligament). The joint is completely disorganized; cartilage is almost entirely destroyed.

4. *Dr Symington* showed a HORIZONTAL SECTION OF THE SKULL, made so as to show the external auditory meatus, the tympanic membrane, the tympanum, and Eustachian tube; also the antrum and the floor of the nose. In addition to the Eustachian tube, the fossa of Rosenmuller was exposed. The specimen showed the edge of the vomer was not always a safe guide in the passage of the Eustachian catheter, inasmuch as it was at least a quarter of an inch in front of the Eustachian orifice. Dr Symington had seen the same condition in other specimens.

IV. Original Communication.

THE PLACE OF SPECIALISM IN GENERAL PRACTICE, WITH REFERENCE TO DISEASES OF THE EYE, EAR, THROAT, AND NASO-PHARYNX.

By George Hunter, M.D., F.R.C.S. Ed., Linlithgow.

Part II.

In a paper read before this Society two and a half years ago, I ventured to indicate what affections of the eye came within the province of the general practitioner as to diagnosis and treatment, and I now desire to bring before you a similar communication with reference to the morbid conditions of the ear, throat, and naso-pharynx. It is not without considerable diffidence that I make this further demand on your time and attention, fully conscious that I have no new facts or observations to offer for your consideration; that however attractive the study of these diseases may have been to myself, it cannot be expected that many of you will regard them from the same point of view; and well aware that since the reading of the former essay an excellent post-graduate course has been established in connexion with this Medical School, where every facility has been afforded for acquiring a practical acquaintance with the diseases referred to; and, Sir, I am especially diffident when I remember what is due to the position and experience of the great majority of the Members of the Medico-Chirurgical Society. But to those who, like myself, have been unable to profit by primary teaching or post-graduate courses, I trust the discussion which follows may not only prove not uninteresting, but stimulate a desire towards the study of a class of diseases which have too long remained a sealed book to the great majority of the profession.

Affections of the Ear.

From what I observed of the practice of my predecessor—one of the most skilful and sagacious surgeons of his day—the treatment of ear affections as carried out by the country practitioner twenty years ago might be summed up in syringing the meatus and blistering over the mastoid; for being without the means of making an accurate diagnosis, it could not be otherwise than empirical and routine. Admitting that Kramer's speculum was to be found in the hands of some practitioners, with only direct light such as is to be obtained in a climate like ours, any diagnosis hereby arrived at could only be misleading and untrustworthy.

It was not for several years after commencing practice that I was able to obtain a good view of the *membrana tympani* and its details, and it has only been by patient plodding and steadily working at the subject that cases of aural disease have been treated with anything like confidence or satisfaction.

It is no part of my intention to take up time by going into all the details of how the examination of a case of aural disease ought to be conducted. These are fully described in all the text-books, and are probably well known to you all; but there are certain points on which I think sufficient emphasis has not been placed, and to these I may briefly allude.

The examination of every case of deficient hearing power should always be tested systematically:—(1), By *conversation;* (2), by *watch;* and (3), by *tuning-fork;* and with ordinary care and patience most cases may by these means be accurately diagnosed. As watches vary very much in the loudness of beat, every practitioner ought to know the distance at which, with normally acute hearing, his own can be heard; and taking 30 inches as an average, the hearing distance for each ear separately should be noted down, with the normal for denominator and that at which the patient can discern it for numerator. This is desirable for purposes of comparison during the progress of the case, and for the information of the specialist, should he require to be consulted.

By the use of the tuning-fork we obtain most important information, and in most cases are able to distinguish between diseases affecting the sound-conducting and the nervous or sound-perceiving apparatus. For this purpose a good large-sized instrument—clamped to subdue the overtones—is necessary. Thrown into vibrations, it ought to be heard more distinctly in front of the meatus than over the vertex or mastoid—*i.e.*, air conduction ought to be in excess of bone conduction. Should the converse be the case, some obstruction to the passage of sound through the meatus, *membrana tympani*, or tympanic cavity exists, but the functions of the acoustic nerve are unimpaired. If, however, bone conduction be impaired or abolished in the deaf or deafer ear, we may assume that the auditory nerve is not so sensitive to the impressions of sound as it ought to be, and that either there exists some abnormal pressure upon the fluid of the labyrinth, or that the nerve itself is diseased (Field). Lennox Browne[1] has called attention to the importance of noting the *duration* at which the vibrations are heard when the fork is placed over the mastoid, and states "that it is of service as indicating whether there is lessened power in the auditory nerve, which is often noticed in catarrhal cases. If the observer, withdrawing the fork the moment the patient ceases to hear the vibrations, place it against his own ear, he can form some idea, according to the time he continues to hear it, of the loss of nerve power on the part of the patient." A good illustration of this statement recently occurred in my practice. I was consulted by Mr A., whose left

[1] *The Throat and its Diseases*, p. 534 *et seq.*

meatus was the seat of ivory exostosis, with H.D. $=\frac{8}{30}$, and bone conduction $-60''$; the right giving H.D. $=\frac{30}{30}$ but air conduction only $-30''$, showing some impairment of nerve power, though hearing distance was practically normal.

I shall now shortly allude to some of the ordinary ear ailments which ought to be successfully treated by the ordinary practitioner, and also to those in which the aid of the aural surgeon should be invoked; and I cannot do better than begin with *cerumen* in the meatus, the treatment of which even the laity feel confidence in undertaking. When I recall the wash-hand basin half full of soapy water, the voluminous towelling round the neck and shoulders, the cunningly-devised ear-scoop, which never did fit closely when and where wanted, the assistant to keep all in position, and the well-drenched collar and neckband, when all was finally accomplished, of the days when I first witnessed this simple operation, I cannot but contrast it with the ease to the surgeon and comfort to the patient with which it is now carried out. An assistant and towels are unnecessary—all that is required being a small bowl, which the patient holds close under his ear, his head being kept quite erect as recommended by Roosa, who also prefers the bowl or finger-glass to the vulcanite receptacle now generally employed.

Though a very simple matter as a rule, Roosa[1] relates a case where, after syringing every day for a week, the plug of wax was not removed until fuming nitric acid had been applied; and I have in my own practice seen cases where syringing had been repeatedly made use of by other surgeons, and yet the meatus not freed from impacted cerumen. On the other hand, as much harm may result from the forcible injection of water against the drum-head when wax is no longer present, and as acute inflammation of the membrane may result from such practice, the safe rule to follow is to examine, by means of speculum and mirror, from time to time, until the meatus is seen to be clear.

A troublesome form of impaction is that caused by layers of laminated epithelium alone or mixed with cerumen. In these cases, as in the ordinary form, removal is expedited by the instillation of bicarbonate of soda. In removing thickened epithelium, some caution is necessary, especially when attempted by forceps; for in such cases the *chorda tympani* may be injured, as actually happened to a relative of my own, with the result that facial paralysis supervened. After removal some astringent or alterative application should be made to the eczematous raw surface of the margin of the meatus, the latter being carefully dried with absorbent wool.

Cases of cough, which had resisted the skill of eminent physicians, and which were cured by removing impacted cerumen from

[1] *Diseases of the Ear*, p. 157.

the tympanic membrane—the irritation being conveyed by the facial and reflected by the vagus—must be known to most of those present.

Amongst the common ear affections for which the practitioner's advice is sought is *chronic otorrhœa.* Either as the result of inflammation conveyed through the tubes in the naso-pharyngeal affections of the exanthemata, or from an acute tympanic inflammation becoming chronic in those of strumous habit, it is at once the most important and the most neglected. It is unnecessary to recall the close anatomical relations the tympanic cavity has to the brain and cerebral vessels; and if the public are not now cognizant of the risks of neglecting such cases, it is not from want of words of warning from the profession.

The treatment of such cases is, I fear, rather routine, the use of an antiseptic or astringent lotion being all that is enjoined. Now, in the great majority of such cases this is insufficient; for inspissated pus and secretions are allowed to remain in the tympanic cavity, and becoming there decomposed, give rise to that disgusting odour which has always appeared to me *sui generis,* and is probably the result of the growth of some special bacterial form. While syringing with antiseptic or astringent lotions, insufflation of impalpable powders with similar properties, and the topical application of rectified spirit, are all very well so far as they go, in many instances they will not effect a cure, unless combined with Valsalva's or Politzer's inflation, which both cleanses the tympanic cavity and prevents adhesions between the promontory and membrane.[1] My own experience has not been so satisfactory with the "dry method," as it is termed, for crusting and retention of discharge, with feeling of tension, have not unfrequently been complained of; whereas warm boric instillations and solutions of carbolic acid and zinc have very generally proved satisfactory. Of the alcoholic treatment recommended by Barr in certain cases I have no experience. The great point to be insisted on, however, is absolute cleanliness and freedom from unpleasant odour, even though only obtainable by syringing three or four times daily.

Not unfrequently the patient does not come before the practitioner until troublesome *granulations* and *polypi* have blocked the meatus. In a case which had lasted upwards of ten years, I removed the latter by Wilde's snare, and the former, with the remains of the polypi, by the instillation of absolute alcohol; but though the discharge entirely ceased, and the general health was greatly benefited, hearing power was not much improved.

In a recent case, after all had been removed by Wilde's snare that could be got hold of, some troublesome flabby granulation tissue was taken away by the application of a paste composed of

[1] *Guide to the Study of Ear Disease* (M'Bride), p. 54.

equal parts of creasote and arsenious[1] acid on the point of a probe. No pain was complained of, and the dead pieces came away a day or two afterwards during syringing. I have also very cautiously employed a fine galvano-caustic point to destroy these tissues, but I was then unaware of the strong recommendations of chromic acid by Drs M'Bride and Barr, and in future I intend to try this application instead.

A still further consequence of chronic otorrhœa is *mastoid disease*, and I recall two cases where sinuses led to necrosed bone, which was satisfactorily removed by operation, and good recovery ensued. I have also to record two fatal cases where, in consequence of suppurative otitis, cerebral symptoms developed, and death from coma supervened. In both careful antiseptic syringing was diligently employed without avail, and as these occurred before the extremely interesting cases of Mr Barker and Professor Greenfield had been published, and also before I was privileged to listen to the able communication read before this Society by Dr M'Bride, no operative procedures were employed.

The next form of aural disease to which I shall refer as claiming the serious attention of the general practitioner is *Mucous Catarrh of the Middle Ear*. It would be difficult to over-estimate the importance of an acquaintance with the causation and course of these affections,—1st, Because of their frequency, for Roosa writes[2] "that out of every thousand cases that present themselves in private practice, one-half are chronic non-suppurative catarrh;" 2nd, Because in their early stages, in the great majority of cases they are amenable to treatment, by treating the cause which produces them; 3rd, Because, if unchecked, they almost certainly lead to incurable deafness; and 4th, Because by their insidious approach (especially in adults), and without many of the usual subjective symptoms of ear disease, they are not diagnosed until they come before the specialist, it may be years after the disease has been in course of progress. I shall best emphasize what I have to say under this head by narrating the history of two cases which have been recently under my care.

W. L., æt. 14, has ever since childhood been liable to catch cold in the head, with a tendency to catarrhal discharge from the nostrils; and it was noticed that he was unable to use his handkerchief to remove it like other children. He snored loudly at nights, and was every now and again liable to attacks of earache. At school he was observed by his teachers, and at home by his parents, to be deaf, but at first not much attention was paid to it. His deafness has gradually increased, and when he came under my observation I found his condition as follows:—

Subjective.—Only loud conversation could be heard by each ear. Watch gave H.D. for right, $\frac{2}{30}$; left, $\frac{4}{30}$. Tuning-fork placed over

[1] A. P. Whitwell, *Pacific Medical and Surgical Journal and Lancet*, July 1887.
[2] *Diseases of the Ear*, p. 258.

the vertex was heard most distinctly in the right ear. Air conduction was $-70''$, and bone conduction $-12''$ on the left side, whilst on the right they were $\frac{a\ c}{b\ c}=\frac{-90}{-30}$. He complains of ringing noises in his ears like bells sounding in the distance. He has at times a dull, rather stupid expression.

Objective.—The meatus is small and much curved. The *membrana tympani* is entire, of a dull bluish-gray colour with a reddish tinge; the short process of the hammer stands strongly out, but the handle is scarcely visible. There is no cone of light to be seen, and the general appearance of the surface is indicative of much indrawing. The pharynx and uvula are relaxed and congested. The right lower turbinated body is greatly swollen, reddened, and in contact with a deflected septum. The middle turbinated is also tumid, but scarcely visible, in consequence of the swelling beneath it. The right nostril is almost completely obstructed, and a similar condition, but to a less extent, also exists on the left. Posterior rhinoscopy was unsatisfactory, but digital examination of the vault of the pharynx discovered no adenoid enlargements. Cocaine spray (8 per cent.) to the right nostril caused marked reduction of the right inferior turbinated, and facilitated inflation by Gruber's method, with the result that H.D. on the right side was at once extended to $\frac{12}{30}$, and in the left to $\frac{8}{30}$. As the cause of the deafness in this case was evidently the obstructed nares and consequent pharyngeal catarrh, treatment was directed towards relieving and removing these conditions; but, as this will be fully gone into under the morbid affections of the naso-pharynx, I shall not here further allude to it.

As giving an instance of the disease in a more advanced stage, and as it occurs in a patient who has reached middle age, I cite the following case:—Mr C. consulted me in consequence of impaired hearing and cold in the head, which he said had lasted for more than two months. On further inquiry, he informed me that two years ago he had suffered from a similar attack, accompanied by troublesome noises in the right ear like kettles boiling. His friends affirm that his hearing has been much impaired ever since, though he does not admit it.

Subjective.—Conversation only heard when carried on in loud tones. Watch gives H.D. $=\frac{1}{30}$ for the right, and $=\frac{5}{30}$ for the left. Tuning-fork on vertex heard much more distinctly in right ear, where duration of air conduction was difficult to estimate, as *tinnitus* prevented him being able to say when vibrations ceased. On left side $\frac{a\ c}{b\ c}=\frac{-70}{-40}$. *Membrana tympani* on both entire; *right* of a dull bluish-gray colour; manubrium much indrawn and foreshortened; short process prominent, pouching in front and behind the handle. On posterior inferior quadrant a patch of whitish opacity, and a cone of light above it. *Left* less indrawn; no patch of degeneration, but other details similar, though less in degree. *Fauces.*—Uvula flabby, swollen, and close to posterior pharyngeal wall. Pillars congested, and posterior part of the pharynx coated with catarrhal secretion. *Nares.*—Right turbinated bodies, lower and middle, vascular, swollen, and nearly in contact with septum, from which a well-marked prominent spine is seen to project into and obstruct the inferior meatus. Left side more patulous ; septum deflected to the right. On asking him to blow through each nostril alternately while compressing its fellow, the right was evidently much obstructed, and along with the expired air intermittent pellets of mucous secretion were expelled with high-pitched whistling sibilus. He was unable to inflate the tympanum by Valsalva's method, nor did I succeed on the right side by Politzer's, but by means of the catheter air could be drawn into both tympanic cavities, as ascertained by the diagnostic tube and the patient's sensations. But though ordinary conversation in rather undertones was now distinctly heard, H.D. was not much increased, and the good results of middle ear inflation only of short duration. Treatment by alkaline nasal spray, astringent gargles, and chloride of ammonium inhalations, was also enjoined ; but the *tinnitus* still continues, and the prognosis is much more unfavourable than in the case of W. L.

These two cases afford good examples of deafness, the consequence of naso-pharyngeal affections, implicating the faucial orifice of the Eustachian tubes, and causing first swelling, then obstruction, and ultimately extending to and involving the tympanic cavity.

Though I do not wish it to be understood that I regard every case of tympanic catarrh as the result of morbid conditions of the naso-pharynx, I am decidedly of opinion that the great majority of such affections are so produced, and I hold strongly that it is the duty of every practitioner, when consulted in cases of impaired hearing, to make a careful and complete inspection of the naso-pharyngeal cavity. It is before him that patients present themselves in the early and, as I believe, curable stages, and he incurs a grave responsibility if from ignorance or incapacity he is unequal to their due recognition and suitable treatment.

In most of the cases of chronic enlargement of the tonsils, and in the adenoid growths of the vault of the naso-pharyx occurring in young people, mucous catarrh of the middle ear may be suspected; whilst nasal polypi, hypertrophic rhinitis, and granular pharyngitis, generally lead to it in the adult. If, on inspecting the *membrana tympani* there be found much indrawing, and if tinnitus be also complained of, treatment should be directed towards restoring the permeability of the Eustachian tubes, and permitting ventilation of the tympanum by means of the air douche.

This is accomplished by Politzer's inflation through the nostrils only, or by means of the Eustachian catheter; and it is of the utmost consequence that the ordinary medical attendant should be practically acquainted with the proper performance of these methods, for they form a most important part of the treatment of these affections in the early stages. In the deafness of children and young people, Politzer's method of inflation is sufficient without the catheter, and in order to make the introduction of the nozzle less uncomfortable, I generally employ a cone of thick indiarubber, such as is recommended by Woakes for sniffing up solutions through the anterior nares. It is not desirable to ask them to swallow water, which, if used, is not unfrequently sputtered over their breast and the operator's arms, and seldom carried into the pharynx when the signal is given for doing so. A better plan in older children and young people is Gruber's method, which consists in asking them to repeat "*hook*" at the moment the bag is compressed, for they find it easier to pronounce than to swallow at the word of command. I need scarcely add that whatever mode of inflation is employed, the diagnostic tube should always connect the patient's ear with his own to verify or contradict his statements.

If by these means no air can be conveyed to the tympanum, then catheterism of the Eustachian tubes should be had recourse to. My own experience is that this is much more easily accomplished, and with decidedly less risk to the patient, than the introduction of a catheter through a tight stricture of the urethra. In consequence of inequality of the walls and projections into the lumen of the inferior meatus, it is sometimes a little difficult to pass the catheter expertly along its floor, as the usual practice should be; but choosing the proper size and curve, it can generally be gently insinuated round the inferior turbinated and past projecting spines until it has reached the posterior pharyngeal wall. Here the beak is turned inwards until it hooks round the vomer, as recommended by Löwenberg, and then by rotating upwards and slightly outwards until the ring looks towards the outer angle of the eye, it will generally be found that the point has entered the faucial orifice of the tube.[1] Inflation is now carefully made by means of the bag in the usual way, care being taken not to cause leverage movement on the distal end of the catheter, and the results estimated as before by the diagnostic tube.

In those advanced cases of non-suppurative catarrh where collections of fluid, proliferation of epithelium, or bands of adhesions have taken place, with stiffening of the articulation of the ossicles and of the connexion between the stapedius and

[1] An inspection of some moist sections, which through the kindness of Dr Symington I was recently permitted to examine, leads me to doubt the correctness of the posterior inferior extremity of the vomer being a reliable guide to the Eustachian orifice in every case, for in three preparations Dr Symington and I satisfied ourselves that the part of the vomer which would be embraced by the catheter when passed in the manner described was at least $\frac{1}{4}$, in one $\frac{1}{2}$ inch in front of the tubal opening.

fenestra ovalis, the case is quite beyond the treatment of the general practitioner, and ought to have the opinion and treatment of the experienced aural surgeon.

Time does not permit me further to refer to the troublesome *tinnitus* which complicates many of the middle ear affections, but I may be permitted to refer to the case of an elderly lady, who sent for me in consequence of having for some time suffered from a roaring sound, like the waves of the sea, in her ears, with deafness and vertigo. So troublesome was it that she frequently found herself reeling like a drunken person, and on the occasion referred to had fallen in the hall with such violence that the whole of one side of her face was blackened and bruised, and her peace and composure of mind rudely shaken. Knowing that she was the subject of extensive vascular atheroma, a passing fear of apoplexy was dispelled by finding a very large and evidently very old plug of cerumen in the meatus, on the removal of which the membrana tympani was found much indrawn, and by pressure on the labyrinth satisfactorily accounted for the tinnitus and vertigo complained of.

Affections of the Throat.

What has already been stated as to the absence of means of instruction in diseases of the eye and ear at the time I refer to applies even more forcibly to those I now propose briefly to bring under your consideration.

Clinical medicine was then taught by one whose reputation in connexion with diseases of the respiratory organs was then unrivalled,—I mean the late Professor Bennett. Those who shared with me the opportunity of profiting by his prelections, will readily recall the enthusiasm he inspired, the time and trouble he took to insure minuteness of examination and completeness of diagnosis, and yet I cannot remember ever to have seen a laryngoscope in his wards, much less its being applied for the elucidation of laryngeal disease. It was only by a process of exclusion by reasoning from analogy, by deeply inspecting the fauces, and in some instances by introducing the finger beyond the epiglottis, that he was able to infer the existence of laryngeal disease.[1]

The laryngoscope was then of too recent introduction to be used even by the teachers of medicine, and it followed, as a matter of course, that their students were ignorant of the advantages to be derived from its systematic employment. To conscientious practitioners this want made itself speedily felt, and I recall the difficulty I encountered in the treatment of my first case, that of J. M., already alluded to in the previous communication to this Society, where a grave affection, the result of tertiary syphilis, was mis-

[1] Bennett, *Clinical Lectures*, p. 648.

taken for a catarrhal condition, the result of repeated exposures to cold.

Believing that the laryngoscope is even now by no means so general as it deserves to be, I have thought a discussion on its advantages might be *àpropos* of the present time, the more especially as the recent illness of an illustrious patient, in whose case wide-spread interest is felt, has forcibly directed not only medical opinion, but also the earnest attention of the general public to the subject.

It would be out of place here to do more than allude to the instrument and its mode of employment. That in most general use is made by Reiner of Vienna, but perhaps a more convenient form is the one with spectacle frame, known as Lennox Browne's, having a mirror of shorter focus, and suits equally well for nose or ear examination. As to the means of illumination, Morell Mackenzie's lamp with rack movement and bull's eye condenser will be found most generally useful.

The examination can only be considered satisfactory when, in addition to the more superficial parts, the entire length of the vocal cords, from their anterior commissure to their insertion into the arytenoids, can be distinctly made out. The special points for the practitioner to note are :—*Colour, i.e.,* vascularity and congestion, or its absence; *form,* especially tumefactions and infiltrations of epiglottis, ary-epiglottic folds, and arytenoids; *ulcerations,* their situation, surroundings, depth, and extent; *mobility* of the cords during inspiration and phonation; and *neoplasms,* their colour, form, and site. In this way we ought to be able to inform our patients whether they suffer from functional or organic disease; and, if possible, in the great majority of cases, what form of the latter.

As illustrating the utility of the laryngoscope to the general practitioner in diagnosing the more ordinary laryngeal affections, I shall read short notes of cases where I have found it of service :—

Mrs S. consulted me two years ago on account of hoarseness, barking cough, pain in deglutition, with sensation of tightness in the upper part of the windpipe on exertion—symptoms which, she said, had lasted for the previous fortnight. Inspection of the fauces revealed nothing abnormal, except slight congestion of the posterior pharyngeal wall, and in front of the left tonsil a white patch on the mucous membrane. Laryngoscopic examination showed marked hyperæmia and slight tumefaction of the left arytenoid, and also of the left ary-epiglottidean fold as compared with those of the opposite side. The left vocal cord was also slightly reddened, and mobility impaired on phonation. The case was evidently one of subacute partial laryngitis, and probably rheumatic. Inhalations of tinct. benzoin co., and the insufflation of pulv. aluminis were prescribed, with good results; for her voice soon regained its natural tone, cough was relieved, and the redness disappeared from the affected part of the larynx. The white patch on the tonsil she was able to explain as follows:—Having consulted a medical man in her own neighbourhood, he examined her throat, and after remarking that he had often seen one much worse, gave it a good touch with solid caustic,—need I add, without much benefit to the symptoms complained of.

Chronic catarrh of the larynx is perhaps the most common throat affection which induces the general practitioner to seek the advice and assistance of the specialist. From its tedious course and resistance to treatment conducted on general principles, as well as from the dread of consumption it often gives rise to, it may cause much anxiety both to doctor and patient.

H. R. came under my care some time ago, complaining of troublesome hoarseness, tickling cough, dysphagia, and a tendency to retching and sickness, which, he says, had been gradually getting worse during the previous month or six weeks. In the morning his voice was rough and harsh, but became clearer after breakfast, to get worse again towards the evening. Though pale and flabby he was well nourished, and his family history showed no tendency to inherited diathesis. On inspection of the fauces the mucous membrane of the pharynx was seen to be dry and granular, with irregular vascularity. Examination by laryngoscope showed the following:—Vocal cords are of a pink fleshy colour, and appear to be slightly thickened; the ventricular bands are reddened and full, whilst the structures forming the upper part of the larynx are decidedly hyperæmic. Mobility of the adductors is slightly impaired, and the inter-arytenoid fold is rather tumid; but there is no infiltration of arytenoids or epiglottis, nor any appearance of ulceration visible. The case was therefore one of simple chronic laryngitis. The treatment consisted of stimulating inhalations of ol. pini sylvestris, and the regular application of a solution of zinc chloride (gr. xxx. and ℥j.) to the interior of the larynx by means of a suitable laryngeal brush, guided by the mirror. The progress of the case was very slow, and treatment extended over two or three months. Tonics with bismuth were administered to alleviate the gastric catarrh, and gradual improvement became apparent.

I am convinced, however, that tonics, expectorants, inhalations, and counter-irritants externally, will avail but little, unless treatment be applied directly to the inflamed laryngeal structures; and to do this efficiently, the mirror must see the brush directed over the interior of the larynx before the spasm provoked by its contact has taken place. By using the mirror with the left hand, the patient meanwhile holding out his own tongue, the general practitioner, after not very long practice, ought to be able to carry the brush into the larynx, keeping in mind the tendency there is to pass it too far backwards instead of dropping it straight down behind the epiglottis. One word of caution as to prognosis in these cases may not be out of place. Dr Hunter Mackenzie[1] has shown that cases of chronic laryngeal catarrh, especially if of long standing, are sometimes liable to drift into laryngeal phthisis. If, therefore, even slight infiltrations or ulcerations should present themselves, a careful examination of the lungs should at once be made.

Regarding *laryngeal phthisis*, some very important observations have been recently made which ought to be of very great interest to those engaged in family practice. It has now been conclusively proved that the larynx may be primarily affected by tubercular disease, and even cause the death of the patient, without the most careful examination of the lungs affording the least evidence of phthisis. Lennox Browne and Hunter Mackenzie both quote the case reported by Demme, where on post-mortem examination tubercle bacilli were found in the larynx, the lungs being absolutely free from disease; and the latter author alludes to five other cases of a similar nature. It is of the utmost consequence, therefore, that the general practitioner should be able to recognise laryngeal phthisis when a case presents itself before him, lest perchance it may happen to be primary; in which case there is the hope that, by using menthol and lactic acid, which have lately been largely used, a cure may be established, and pulmonary infection avoided. Marked anæmia of the laryngeal structures is the earliest sign to excite anxiety; and if followed by infiltration of the arytenoids, ary-epiglottic folds, or epiglottis, the diagnosis may be almost considered established. The following is an instance of a well-marked case:—

D. R. came under my care last summer suffering from dysphonia, cough, pain on deglutition, shortness of breath, and the usual symptoms of phthisis. Examination by laryngoscope showed that both vocal cords were the seat of ragged, shallow, worm-eaten ulceration, whilst the arytenoids and ary-epiglottidean folds presented the usual well-marked, pear-shaped infiltration and tumefaction. In such advanced cases the appearances cannot be mistaken, the more especially if the under surface of the epiglottis be also ulcerated. Here there is much pain and great distress in swallowing even liquid nourishment, and in small quantities. This was the condition of W. R., who was under my care during the autumn, and who was able to swallow for a time more easily if he took liquid support while leaning over the edge of the couch with the face and chest prone. Applications of cocaine to the sensitive, inflamed, ulcerated parts were productive of much relief, when the insufflation of morphia with bismuth proved valueless.

Syphilitic affections of the larynx are fortunately, or unfortunately, by no means of frequent occurrence in my part of the country, and the opportunity of noting the laryngoscopic appear-

[1] *Edinburgh Medical Journal*, January 1887.

ances does not frequently present itself. The early stages of secondary syphilis may undoubtedly mislead, but in the advanced tertiary stages, where such havoc is made of the epiglottis and arytenoids by rapid and deep (roundish) ulcerations, the diagnosis ought to be one of no great difficulty. In the case of J. M. referred to, death ensued from stenosis, the consequence of adhesive inflammation and œdema, the fatal termination taking place a few minutes before reaching his house, whither I had gone to perform tracheotomy. The absence of marked pain, the history of the case, and the results of specific treatment, will still further prevent these diseases of the larynx being confounded with the tubercular, even at the hands of those not devoting themselves to the special study of such ailments.

I cannot do more than refer to the valuable information derived from the employment of the laryngoscope in laryngeal paralyses, especially in those of the *recurrent laryngeal*, where the presence of an aneurism of the aorta, of enlarged bronchial glands, or of cancer, may be diagnosed by inspection of the larynx, and noting the mobility of the cords in phonation and inspiration. In this connexion it is only just that Semon should have the credit of having pointed out that by far the most frequent and important form of recurrent paralysis is that of the abductors, especially the left, the consequence of paralysis of the left posterior crico-arytenoid.

Nasal Affections.

No apology is necessary for offering some remarks urging the importance to the general practitioner of an acquaintance with the morbid conditions of the nose and naso-pharynx. Ophthalmic surgeons are well aware of the connexion between epiphora and obstruction of the nasal duct by the pressure of polypi in the inferior meatus; and Woakes has called attention to the defects of vision and reflex disturbances of the conjunctiva and fundus oculi in conjunction with ethmoiditis. The same author writes,[1] "that deafness, sometimes accompanied by tinnitus or vertigo, or both, exists in about 70 per cent. of the cases observed by him." I have already referred to the frequency with which non-suppurative catarrh of the Eustachian tube and tympanum can be traced to diseased conditions of the nose. Loss of the sense of smell and, secondarily, of taste in connexion with nasal obstruction is too well known to require more than a passing notice. But the observations of Voltolini, Hack, Zuckerkandl, Sommerbrodt, Mackenzie, Fraenkel, Schäfer, and Hering, quoted by Dr M'Bride,[2] go to show "that in a certain proportion of cases such affections as asthma, bronchitis, migraine, neuralgia, and even epilepsy, may be

[1] Woakes, *Nasal Polypus*, p. 37.
[2] *British Medical Journal*, Jany. 29, 1887.

due to chronic change in the nasal mucosa, and can be cured by treatment directed to the affected parts." If, then, the senses of sight, smell, taste, and hearing may become impaired, and such grave diseases of the nervous and respiratory systems can even only occasionally be found to have their origin in diseased conditions of the nose, no great amount of argument is necessary to commend these ailments to more serious consideration than they have hitherto received. It has only been recently that the study of nasal affections has been brought prominently forward; and believing that by the great majority of practitioners little attention is devoted towards their diagnosis and treatment, I have deemed it worth while to make a few observations under those heads.

My first case after commencing practice was not attended by very happy results to the patient or much satisfaction to myself. I was consulted by a laundrymaid on account of nasal obstruction of considerable duration. Examination in front of the window by raising the tip and dilating the nostrils as best I could, showed a grayish-red shining swelling low down in the right meatus. Believing this to be a nasal polypus I made sundry attempts at its evulsion by ordinary dressing forceps, with no other result than the infliction of very considerable pain, and a rather free amount of bleeding. These compelled a cessation of my efforts, and it then dawned upon me that the inferior turbinated body was the sphere of my operations, and a request to come again for further treatment did not meet with a hearty response. This mistake may, I believe, be made by any one who has not devoted some attention to the examination of the nares, and more especially if unprovided with suitable instruments for the purpose.

I shall briefly mention those aids to diagnosis and treatment which I have found serviceable in my ordinary practice. The chair on which the patient is seated should be provided with a head-rest similar to those found in dentists' rooms, or support may be obtained by elongating a narrow chair back upwards. The former is what I employ, and is the more satisfactory arrangement, especially where operative procedures are found necessary. The mirror and lamp are those used in the examination of the throat and ear, and they suit all three purposes sufficiently well.

Regarding the speculum a word or two more is necessary. The form I have found to answer every purpose, both as to comfort to the patient and excellence of illumination of the deeper parts, is that known as the Duplay-Charrière, and this is also the one for which Moldenhauer[1] expresses a predilection. The only drawback to its use is not being self-retaining, and this is occasionally a hindrance to its employment. That invented by Lennox Browne remedies this defect, but with it illumination is deficient. A probe is also required to ascertain the sensibility of the mucous

[1] Moldenhauer, *Die Krankheiten der Nasenhöhlen*, p. 29.

membrane—*e.g.*, if cough or sneezing reflexes, and the mobility of any morbid growths. For this purpose that recommended by Cresswell Baber, with flat handle bent at an angle, is very suitable. Thus provided, the practitioner should be able to make a complete inspection of the anterior nares, and recognise what he sees. He should especially note condition of the septum as to deviation and spinous projections, whether the anterior turbinated body be in a state of simple vascular erection, or the seat of hypertrophic enlargement; whether middle turbinated bone in contact with septum, and if its mucous membrane be much thickened, also if the seat of new growths on its inner convex or outer concave surfaces, or upon its anterior or inferior borders. Finally, he should ascertain if there be free breathway with closed mouth, in either the recumbent or erect position, or after the use of stimulants, and if so, no matter what the internal confirmation may be, the nares may be considered normal as to function. But to make the examination complete, posterior rhinoscopy must be had recourse to. This is in many cases difficult, and in some impossible, but it should always be attempted where anterior inspection does not explain the cause of obstruction or other morbid condition. Where the post-palatal space is ample the posterior rhinoscopic image is obtained without difficulty, by using the smallest sized mirror in the laryngeal case. It is otherwise when the uvula is close to the posterior pharyngeal wall, and where the fauces are irritable and retching easily produced. Here the small post-rhinal mirror of Michel[1] of Köln, constructed in such a way that the reflecting part is introduced horizontally, and when in position gradually brought to a right angle with the handle, by means of a trigger arrangement in the bent handle, is of decided advantage, being much more comfortable to the patient and more easily managed by the surgeon.

I find the angular tongue depressor recommended by Baber[2] the most convenient, and in using it or any other form it is of consequence to depress the base of the tongue downwards and forwards, as strongly insisted on by Moldenhauer,[3] for in this way the working space is materially increased. In difficult cases the application of cocaine to the posterior pharyngeal wall will prove of service. Directions are generally given to breathe through the nostrils after opening the mouth wide, and if the palate tends to rise, to pronounce "on," "hang," etc. My short experience leads me to state that the less instruction of this kind the better, for thereby muscular contraction is provoked, and the object in view defeated. By making the least possible fuss, and slipping the small mirror quietly in by the side of the uvula without remark of any kind, the more likely will success be attained. One word

[1] *Die Krankheiten der Nasenhöhle*, p. 14 (American translation).

[2] *Guide to the Examination of the Nose.*

[3] *Op. cit.*, p. 34.

of caution as to the interpretation of what is seen by beginners may not be out of place. The posterior extremity of the inferior turbinated body may often be seen irregular and uneven on its surface, resembling a small nutmeg, and this, which is its normal appearance, may be taken for hypertrophic degeneration,—a mistake which I confess to have fallen into.

The presence of polypi, enlargements of septum, adenoid vegetations, and especially of pharyngeal tonsil, should be looked for. At the same time careful inspection of the Eustachian orifices, with their anterior and posterior folds, should be made.

It only remains now to refer to the most important morbid condition of the nose for which medical aid is sought, viz., *nasal obstruction*, its symptoms and the causes which produce it. I have already referred to headache, asthma, and neuralgia as being, in some cases, the consequence of nasal stenosis, and I shall now quote from Greville Macdonald's interesting and suggestive *brochure*[1] some of the other symptoms it produces. "The patient complains of stuffiness in his nose, chronic cold, and inability to clear his head, although he uses his pocket handkerchief constantly. He snores at night, his voice is unresonant, he has difficulty in mastication and swallowing, he is possibly anæmic, and otherwise badly nourished. He has a peculiar physiognomy, varying from a slight elevation of the upper lip and alæ nasi to complete buccal respiration and pinched nose. He may suffer from a dry throat and hoarseness, or aphonia. Finally, there may be no direct symptoms at all." A simple method of observing the relative obstruction is to ask the patient to repeat M N while holding a mirror in front of the nares, and noting the amount and extent of vapour which condenses on the mirror's surface as emitted from each nostril. The same object may be obtained by using one of the chloride of ammonium inhalers, and observing the volume of fumes expelled from the nostrils separately. Obstructed nares may result from any of the following conditions,—(1), *Chronic rhinitis*; (2), *nasal polypi*; and (3), *post-nasal growths.*

The two following cases of hypertrophic rhinitis have been recently under my care:—

Jessie X. sought advice in the autumn in consequence of obstruction of the nostrils and deafness, which had lasted for several years. She had consulted several medical men, one of them being a well-known aurist recently deceased, and at their hands her ears had received the most approved treatment; but no examination of her nares had been made. Her drum heads were both indrawn, and her hearing power was much impaired. The right nostril was found almost completely occluded by a very much hypertrophied inferior turbinated body, the surface of which was red, irregular, and resisting to the probe. A deeper view of the parts

[1] *The Forms of Nasal Obstruction*, p. 19.

was rendered impossible by its bulk, and also by the septum being deflected to the right. Cocaine spray, 8 per cent., caused very little diminution of its bulk, only sufficient to permit a view of the middle turbinated body, which was found in contact with the septum, and its mucous membrane apparently thickened. On the left side the lower turbinated body was scarcely at all enlarged, but high up the middle spongy bone also impinged against the septum. The pharyngeal mucous membrane was tumid, and there was a considerable amount of post-nasal catarrh with implication of the orifices of the tubes, but posterior rhinoscopy showed nothing abnormal, excepting a crowding up of the choanae. The right lower turbinated body was freely cauterized by a von Bruns flat platinum burner, and after contraction had taken place, the breathing on the right side was established with decided comfort to the patient. Hearing also improved considerably under treatment, but the case was of too long standing to give expectation of hopeful results.

R. L.'s, already alluded to under affections of the ear, was a much more satisfactory one, and illustrated well the results of treatment directed both to the ear and naso-pharynx. The inferior turbinated body on the right side could be pushed aside and dimpled by the probe to a certain extent, and cocaine when applied caused a very decided shrinking, thus distinguishing its erectile tumefaction from true hypertrophy. Throwing the head still further back showed the septum encroached upon by the middle spongy bone on both sides, and, lowering it, the inferior meatus patulous only to a limited extent. Mucous catarrh was abundant from the anterior as well as the posterior nares. Alkaline antiseptic spray, of boric acid and borax, for the anterior nares and the naso-pharynx was actively treated by the free application of zinc chloride. The permeability of the inferior meatus was increased by the regular passage of nasal bougies, beginning with a No. 6, and gradually increasing the size up to 10. Under this combined treatment his H.D. on the right extended from 2 to 30 inches, and on the left from 4 to 18 inches, ordinary conversation being heard without the slightest difficulty. The handle of the hammer was also found much less indrawn, though still greatly congested, and the *membrana tympani* assumed a much more natural colour and curvature.

Regarding the treatment of *nasal polypi* much difference of opinion now exists. Evulsion by forceps, removal by cold snare, and the use of the galvano-cautery, have each their enthusiastic supporters. Sir Morell Mackenzie[1] writes that "the great advantage of evulsion if not only the *facility* with which the treatment can be carried out, but the *rapidity* with which relief can be obtained. More growths can be removed at a single sitting than can be got rid of either with the snare or by electric cautery." On the same

[1] Morell Mackenzie, *Diseases of Throat and Nose*, vol. ii. p. 376.

page, however, he states, "I feel convinced that no practitioner would ever allow evulsion to be performed on himself."

The cold snare is that recommended by Moldenhauer,[1] Schech,[2] Woakes,[3] Greville Macdonald,[4] and I believe by Dr M'Bride; and probably is the method most generally used. Galvano-cautery has been strongly advocated by Voltolini,[5] Michel,[6] Thudicum, and Wolston,[7] principally on the ground that where the polypi are somewhat tough, or have a broad base, the wire of the cold snare breaks, for one has to pull pretty hard, and a very copious hæmorrhage follows.

I have operated by all these methods, and regarding the first, I can only say that I should never allow forceps, no matter how skilful the hands employing them, inside my nares; and what I do not tolerate in my own case, I could certainly not have recourse to for my patients. Believing, however, that polypi are only the *symptoms* of a morbid subjacent condition, and not the disease itself, it matters the less whether they are removed by cold or galvano-caustic snare, provided the deeper seated cause is energetically treated. But as the cold snare is of much simpler construction, less expensive, and in many cases very effective when followed by the application of chromic acid, as recommended by Hering,[8] it is probably that form of treatment which will commend itself to the majority of general practitioners of the present day.

While this may be so, from the short experience I have had of the galvano-caustic snare, I am of opinion that it is the more satisfactory, and, when the pathology is kept in mind, the more scientific mode of treatment. It is not my intention to detain the Society with any theories regarding the etiology of nasal polypi, but I am inclined to subscribe to the views of Greville Macdonald,[9] that in a large proportion of cases changes of an inflammatory character take place in the muco-periosteum, and even in the middle spongy bone itself, the polypi being the ultimate development of the hyperplastic and metaplastic conditions. From the statement that necrosing *ethmoiditis* exists in every case, as asserted by Woakes, most observers will, I believe, dissent, for single polypi may be met with surrounded by apparently healthy mucous membrane. That the dependent position of the turbinated mucous membrane, the presence of abundant moisture, and a feeble

[1] *Op. cit.*
[2] *Diseases of Throat, Mouth, and Nose.* Translated by R. H. Blaikie.
[3] *Op. cit.*
[4] *Op. cit.*
[5] *Die Galvanokaustik.*
[6] *Op. cit.*, p. 62.
[7] *Nasal Polypi: their radical Extirpation and Cure by Electro-Cautery.*
[8] Hering, "Ueber die Anwendung der Chromsäureätzungen bei Krankheiten, der Nasenhöhle." *Transactions of International Medical Congress.* Copenhagen, 1884.
[9] *Op. cit.*

circulation, are also not unimportant factors, could, I believe, be substantiated did time permit. Most surgeons are agreed as to the value of the cautery in the treatment of caries in such positions as the knee, wrist, spine, etc.; and one is justified in the inference that a similar mode of treatment would prove not less efficacious in the osseous hyperplasia which has been shown to occur in the middle turbinated bone by the authors referred to. The application of the galvano-cautery checks the hyperplasia which is liable to lead up to polypus, and by destroying the base after it has formed, causes the development of a higher form of embryonic tissue — the fibrous or cicatricial — from which myxomatous growths are less likely to originate, and recurrence less liable to take place. In making these applications the services of the specialist should generally be requested, as there is risk of synechiæ resulting between the septum and middle turbinated, if the cautery be unskilfully employed. The following case, that of Mrs M., is interesting as having been operated upon by all three methods,—five times by forceps, thrice by cold snare, and once by galvano-cautery— a dozen or more of polypi being removed on each occasion. This tells its own tale as to the inefficiency of the first two methods, and her own impression is that they have never been so thoroughly removed as by the battery. The senses of smell and taste have to a certain extent returned since the last operation.

In the case of Mrs G., at two sittings I was able to clear the nares of numerous polypi by galvano-cautery on the one side, and by cold snare on the other; and though the latter was the easier, the absence of bleeding and painful tug, and the readiness with which the flat burner could be applied to the affected area after the use of the hot snare, made a decided impression on my mind in its favour.

Polypi projecting into the post-nasal space do not come within the domain of the general practitioner.

Post-nasal growths, in the form of adenoid vegetations and hypertrophy of Luscha's tonsil, are the last cause of nasal obstruction to which reference will be made.

In the case of D. A., the diagnosis was easily made by posterior rhinoscopy, and their removal was accomplished by Löwenberg's post-nasal forceps, some nasal polypi being at the same time removed.

Whilst nasal stenosis with obstruction thus gives rise to endless troubles, *atrophic rhinitis* with the opposite condition also causes great discomfort to those who are the subjects of it. In most cases it follows after the hypertrophic form, and is accompanied by wasting and thinning of the nasal mucous membrane, and in many cases of the turbinated bones as well, whilst the dried and altered secretion gives rise to the fœtid-smelling incrustation so characteristic of the complaint. Air at an abnormally low temperature and loaded with irritants of all kinds is permitted to pass through the patulous nostrils, and impinging upon the posterior pharyngeal wall originates *pharyngitis sicca*, which by continuity sets up a similar condition in the larynx.

Two patients suffering from this complaint have been recently under my care, and short notes of their cases contain some points of interest.

Jessie C. consulted me in the beginning of the winter on account of a dry condition of her throat and nose, with the formation of crusts having an offensive odour. She had also discomfort in swallowing hot liquids, and at times roughness of voice. Her general health was considerably impaired, and she had a thin, anæmic, pinched look of face. She had sought advice from several practitioners, and had attended at two general hospitals, but no examination of her nose had been made by speculum or mirror. Part of her treatment had consisted in the administration of charcoal biscuits for the relief of what was regarded as a dyspepsia with fœtid eructations, but no good results followed. Anterior rhinoscopy showed the usual patulous nares and wasting of all the textures, with a large extent of crust formation. The pharynx was dry, and its mucous membrane thin and glazed. She was ordered a cleansing alkaline lotion of carbolic acid and soda, and subsequently of boric acid and borax to remove crusts, and also an ointment of iodol, eucalyptus, and vaseline to the naso-pharyngeal mucous membrane. After a fortnight's treatment by this method the odour disappeared, and the tendency to crust formation was scarcely at all noticeable. She soon afterwards returned to her home in every way improved.

Mr D.'s case presented very much the same objective symptoms, the crusts being, however, more dense and sometimes mixed with blood, but without much unpleasant odour. Great discomfort was caused during the night by the diseased secretion dropping down into the larynx and preventing sleep, and also by the necessity for removing the incrustations, which he generally accomplished by passing the forefinger up behind the palate. The same lotion was applied to cleanse and clear away the crusts, after which menthol dissolved in olive oil, in the proportion of 1 to 4, was freely applied to every part of the naso-pharynx.

The case is still under observation, but much comfort has been obtained, the feeling consequent upon the application of the menthol being described as peculiarly cool and refreshing.

For the general practitioner to be able to recognise (if not to treat) the more important diseased conditions of the eye, ear, throat, and naso-pharynx, can only result in good to all concerned. 1*st*, To the patient, because his ailment is diagnosed in the early and therefore more likely curable stage; 2*nd*, To the specialist, because more cases will be sent to him for treatment; and, 3*rd*, to the practitioner himself, whose reputation would not certainly suffer from having been able to point out to his patient what was the complaint under which he laboured, even though he did not feel justified in attempting to deal with its treatment. When so much minuteness of investigation is now being insisted upon, it does seem extraordinary that a practical acquaintance with the diagnosis and treatment of such important organs as those of special sense is not insisted upon by the examining authorities; and I am convinced the student will not be thoroughly equipped until attendance upon courses of these diseases is compulsory, and his knowledge of them tested at the ordinary clinical examinations. There can be little doubt that he will make the best practitioner whose knowledge of special diseases is most extensive, and the most accomplished specialist will be he who is thoroughly well informed in all the ordinary departments of medical science.

Finally, I desire again to express the sense of my obligation to those holding special hospital appointments, or otherwise possessing specialized information, for the never-failing courtesy and invaluable assistance which I have invariably received at their hands.

Dr Mackenzie Johnston, in the course of a few remarks, asked if Dr Hunter had observed any association of pigeon-breast with disease of the tonsils and pharyngeal tonsils. Schech and other authorities said there was a causal relationship between them. After a prolonged investigation in the out-patient department of the Sick Children's Hospital, he had come to the conclusion that, though they might be found occasionally associated in the same individual, there was no such causal relationship as was asserted by these authorities.

Dr P. A. Young contrasted the treatment of nasal polypi of to-day with that which obtained when Dr Hunter and he were students. He thought the difference was between barbarism and civilisation. He thought the electric cautery in which the current was supplied by storage cells was more reliable than galvano-cautery. The difficulty in country practice was getting the accumulators charged.

Dr James Ritchie mentioned two cases in which symptoms of insanity appeared to be caused by the impaction of wax in the external auditory meatus. On this being washed out, recovery took place in both cases.

Dr Maxwell Ross drew attention to an easy method of deciding whether a case was one of chronic catarrh of the larynx or of tubercular laryngitis. It consisted simply in examining the laryngeal secretion for the presence of tubercular bacilli, which could be wiped out of the larynx by means of the brush. This had been done by Dr Hunter Mackenzie and himself, and they had been able to satisfy themselves that some of the cases that appeared to be catarrhal were really tubercular.

Dr Hunter, in reply, said he was aware of the method mentioned by Dr Maxwell Ross, but had omitted to refer to it and a number of other interesting points, as they would have considerably lengthened his paper. One could not generalize with regard to the connexion of pigeon-breast and disease of the tonsils, but so far as his experience went his views were in accordance with those expressed by Dr Mackenzie Johnston. He desired to thank the Society for the cordial reception which had been accorded to the paper.

Meeting IV.—February 1, 1888.

Dr John Smith, *President, in the Chair.*

I. Election of Ordinary Member.

John M'Fadyean, M.B., was elected an Ordinary Member of the Society.

II. Exhibition of Patients.

1. *Dr Scott Lang* showed a girl, aged 9, suffering from a rare example of SALIVARY FISTULA. The external opening was situated about half an inch behind the lobule of the right ear, and from it a clear fluid was seen to trickle. A portion of the fluid, collected and treated with starch and Fehling, gave a pretty large precipitate of suboxide. The fistula had existed since the child was 2 years of age, and had appeared after the occurrence of an abscess in that situation. As it was inconvenient, and sometimes started an eczematous rash, Dr Scott Lang had arranged to place her under Dr John Duncan's care for operative treatment.

2. *Mr Miller* showed a young man who had been about a year ago the subject of a DOUBLE FRACTURE OF THE RADIUS. He had fallen in the harvest-field, and was taken to a bonesetter, who put in what he called a "bone out." The wrist was then treated by poulticing. As a result osteo-myelitis, followed by necrosis, occurred in what was found to have been a case of simple Colles' fracture. After treatment for this condition a spurious ankylosis of the elbow was found to have occurred, and on examination there was commencing gelatinous disease. The joint was excised, and it was then discovered that he had sustained a fracture through the neck of the radius. A very perfect reproduction of the elbow-joint had taken place.

3. *Dr Cotterill* showed a man on whom he had operated for a HYDROCELE of large size by incision, instead of the old method of

tapping and injection. He considered the incision greatly superior to the old method, which in his opinion was clumsy, uncertain, tedious, and unsatisfactory. In this case and others he had after incision swabbed out the interior of the sac with a cloth dipped in iodine, and stitched in a short drainage-tube. He had on one occasion seen sloughing after the injection of iodine.

Prof. Chiene said his experience differed from that of Dr Cotterill. There were some hydroceles which a simple tapping would cure. In the case of chronic hydroceles with large testicles, he had not had any unsatisfactory results from tapping and injection. The points he attended to were to save pain by having the cord grasped firmly during the injection, and to cover thoroughly the whole inner surface of the sac with the Edinburgh tincture of iodine, which was the proper preparation to use. He had never yet seen the inflammation set up pass the plastic stage. He believed the incision plan to be an excellent one, but for Dr Cotterill to recommend it generally was very much the same as if Sir Joseph Lister recommended every one to cut down on fractured patellæ and suture the fragments.

IV. Exhibition of Specimens.

1. *Dr Caverhill* showed the BRAIN and PORTIONS OF SKULL of a case of gun-shot injury from the accidental discharge of a revolver. The points of interest as negativing the probability of suicide were the absence of blackening of the skin about the point of entrance, along with the nearly horizontal track of the bullet. The point of entrance was in front of the right auricle, while the skull had been fractured but not pierced on the opposite side behind the left ear. The patient was unconscious throughout, and there was complete absence of any movement of the voluntary muscles. Respiration became gradually slower. The pulse was 80, regular and firm until a few seconds before death.

2. *Mr Miller* showed a DRESSING removed recently from the patient whose case he referred to at a previous meeting as having undergone amputation for syphilitic disease of the knee-joint. The dressing was one of sublimated wood-wool, and when removed from the patient was found to be stained green, though the discharge was not green. The only explanation he could at present think of was, that the patient having taken large quantities of iodide of potassium, the green iodide of mercury had formed.

The President suggested that the dressing should be analyzed.

V. Exhibition of Instruments.

Dr Cotterill showed INSTRUMENTS for use in the operation for cleft palate.

VI. Original Communications.

1. EMPYEMA OF THE SUPERIOR MAXILLARY ANTRUM WITH ONLY NASAL SYMPTOMS.

By P. M'Bride, M.D., F.R.C.P. Ed., F.R.S.E., Surgeon to the Ear and Throat Department, Royal Infirmary, and Lecturer on Diseases of the Ear and Throat, Edinburgh School of Medicine.

Among both surgeons and specialists there has been, and I may even say there still is a tendency to regard suppuration of the lining membrane of the maxillary antrum as invariably attended with pain, swelling, and distension of the osseous walls of the cavity. The condition to which I propose now to direct attention is associated with none of these symptoms, and the patients usually come to the surgeon complaining only of a constant or periodic discharge of pus from the nose. Sometimes the secretion is offensive, and then both its smell and taste may be perceived by the sufferer; in other instances, however, it is free from fœtor. Inasmuch as one antrum only is commonly affected, the discharge is usually found to come from one nostril,—although it must not be forgotten that as the two nostrils communicate posteriorly any large quantity of secretion finding its way into one may in part escape by the other, more especially while blowing the nose. I have laid some stress upon this point because the unilateral character of the discharge is often only discovered after careful questioning.

Very often, too, the patient has never observed that the quantity of discharge is altered by changing the position of the head, although I believe that a careful rhinoscopic examination will commonly prove that this phenomenon is present. In most instances inquiry will elicit a history of faceache, sometimes extremely slight, or at least of a period of discomfort in the region of the cheek; but this is frequently only discovered by strict questioning, because to the patients there seems to be no connexion between the two conditions.

For the most part there is neither pain nor discomfort when the case is examined, because the suppuration has become chronic, and the pus discharges freely through the natural orifice (or orifices) into the nose. Fraenkel,[1] however, states that in not a few cases frontal headache is complained of, and I have seen the same symptom in one of my own cases. I was at one time inclined to regard this symptom as a reflex neuralgia, but am now more disposed to adopt the explanation suggested by Killian.[2] It will be remembered that the normal opening of the antrum into the nose is close to that of the frontal sinus. We shall presently —when discussing the objective signs—see that the nostril corre-

[1] *Berlin. Klin. Wochens.*, No. 16, 1887.
[2] *Monatsschrift für Ohrenheilkunde, etc.*, Oct. and Nov. 1887.

sponding to the affected side usually shows swelling of the mucous membrane. As a sequence of this tumefaction closure of the communication between the frontal sinus and the nose occurs with the natural result, that as the contained air is absorbed, the frontal cavity is placed under abnormal physical conditions. It is even conceivable that in this way secondary inflammatory changes and exudation might result in the frontal sinus. The possibility of forehead pain and tenderness being due to antral disease is a matter of great importance, as its presence might easily lead to the diagnosis of suppuration in the frontal or ethmoidal cells.

No doubt the class of cases we are considering have been long since recognised by individual surgeons, and probably also by dentists. This may be gathered by a careful perusal of such authors as Erichsen,[1] Heath,[2] Lefferts,[3] Schech,[4] Salter,[5] and others.

Neither in the works just referred to, however, nor in others, have I found this subject treated of with the care and consideration which it merits. Thus, Moldenhauer,[6] who has written the most recent and one of the best text-books on the nose, describes under the designation "Disease of the Osseous Portion of the Nose," a class of cases, in which, despite his reference to Ziem's paper, he seems to assume that the nasal discharge is due to limited caries. Of the six cases there referred to, I believe, from the author's observations, that in every one the symptoms must have been due to chronic suppuration of the lining membrane of the antrum of Highmore. I have stated these facts to show that until quite recently, suppuration of the antrum without retention of pus was in many cases overlooked. To Ziem,[7] therefore, we must give the credit of first directing the attention of specialists to the important fact that a discharge of pus from the nose is often due to suppuration within the antrum. It seems to me, however, that we cannot hold this authority guiltless of extreme zeal for the neglected cause which he espoused. He was, however, able to show a large number of successful results, although he seems to have opened the antrum in almost every case of purulent discharge from the nose for which he was consulted. The most scientific contribution to this question is from the pen of B. Fraenkel.[8] As he very properly points out, the pus in these cases can be seen to issue from the region of the hiatus semilunaris. If then the nose be thoroughly cleansed, and the secretion be seen soon afterwards to appear in the middle meatus, we may suspect antral mischief; but, as

[1] *Science and Art of Surgery*, 1887, vol. ii. p. 474.
[2] *International Encyc. of Surgery* (Ashhurst), vol. v.
[3] *Ibid.*
[4] *Diseases of the Mouth, Nose, and Throat.* Translated by Blaikie, 1886.
[5] *Dental Pathology and Surgery*, 1874.
[6] *Krankheiten der Nasenhöhlen*, etc., 1886.
[7] *Monatsschrift für Ohrenheilkunde* (Feb., March, April, 1886).
[8] *Berlin. Klin. Wochens.*, 16, 1888.

Fraenkel remarks, "Not all pus that is poured into the middle meatus through the hiatus comes from the antrum;" for it may also proceed from the frontal sinus. In order to differentiate the two conditions, he carefully cleanses the nostril and makes the patient sit with his head down, a position which would render drainage from the frontal sinus into the nose impossible. If after this more pus be found, he concludes that the maxillary sinus is at fault.

Let us now take a final glance at the clinical feature of the affection we are discussing. The patient complains of a unilateral discharge of pus, which sometimes trickles from the nose in a continuous stream, and may be fœtid or sweet; in some persons the discharge seems to occur periodically. Careful examination of the affected nostril shows that the secretion comes from a point just below the anterior extremity of the middle turbinated body; sometimes, however, it is sufficiently copious to cover the posterior surface of the palate, posterior end of the inferior turbinated body, etc. If the nostrils be syringed out, and the drop of pus wiped away from the middle meatus, and the head thrown forward with the forehead down, the secretion is seen, on examination undertaken immediately afterwards, to be increased in quantity. Anterior rhinoscopy often shows the mucous covering of the inferior and middle turbinated bodies to be hypertrophied, or at all events in a state of erectile swelling. This last named condition can, of course, be temporarily overcome by the application of cocain or menthol—a proceeding which is often necessary to obtain a better view of the parts.

Posterior rhinoscopy is merely useful in excluding other conditions. The diagnosis of the form of empyema of the antrum under discussion is arrived at by a process of exclusion. The frontal sinus is eliminated by Fraenkel's method, and besides this cavity is rarely affected by itself. Thus Zuckerkandl,[1] whose work on the normal and pathological anatomy of the nose is probably the most complete in existence, has never met with isolated inflammation of the frontal sinus. A careful rhinoscopic examination will exclude all other possible causes of unilateral nasal discharge, *e.g.*, the presence of a foreign body or rhinolith, ulceration, inflammation of the bursa pharyngea, ozæna, caries, etc.

There is, however, one condition which deserves more than passing notice, and which was first pointed out to me by Dr Cotterill, in a case where he operated at my suggestion, viz., a marked redness of the gum corresponding to the affected antrum. It was found in the last case but one which I have examined; in the last the same phenomenon was present. I am not aware that this condition has been so far observed by the few authors who have written on empyema of the antrum without the usually described symptoms. As to the cause of the form of antral affec-

[1] *Normal and Path. Anat. der Nasenhöhle*, etc., p. 168.

tion under consideration, I think by far the most common is to be sought in the presence of decayed upper teeth, usually the bicuspids or first molar. I have met with one case, however, in which the antral inflammation was set up by either long continued nasal catarrh, or by douches used for its cure.

In conclusion, I shall briefly record two cases which seem to me fairly typical.

Mr C. consulted me in August 1887, and has had a discharge of matter from the left nostril for five years or so, and all remedies so far recommended having proved useless.

Objective Examination. — *Post - Rhinoscopy.* — Parts fairly healthy, except some thickening of posterior extremities of the inferior turbinated bodies, especially on left side.

Anterior Rhinoscopy.—Right nostril almost normal; left, pus coming from anterior part of middle meatus, increased by holding head over to the right. No pain or swelling over antrum, but has had a gumboil repeatedly over the stump of the second bicuspid, where there is now a sinus. The tooth was extracted on my recommendation by a dentist (Mr Watson), and the antrum opened through the socket, a proceeding which gave escape to a considerable quantity of pus. Systematic irrigation, carried out by the patient, freed him from his troublesome and distressing affection.

Mr M. consulted me 18th December 1887. For nine years patient has used a nasal douche as part of his toilet; this was ordered on account of an ear affection; occasionally during its employment a little pellet of mucus came away. He has now since July complained of fœtid discharge from the right nostril. There was no history of pain in the antrum; but once during a railway journey he felt a curious drawn feeling in his face. He thought little of this, and it immediately passed off. In this connexion it may be stated that the patient is extremely neurotic, and would probably feel pain acutely.

Present Condition.—Slight tenderness over right cheek on firm pressure; teeth quite perfect; discharge increased by bending head over to left, and also keeping forehead down.

Post-Nares.—Some purulent discharge; no ulcer or other condition to account for its presence.

Anterior Nares.—Left nostril, some catarrh; no pus. Right nostril, inferior turbinated body in a state of erectile swelling, which subsides under cocain; pus coming from middle meatus, and showing pulsation.

Nostrils.—Blown separately, right yields copious fœtid purulent discharge; from the left there comes only serous sweet discharge. No tenderness of the teeth; congestion of the upper gum to the right of the middle line; no bulging anywhere.

On the 22*nd December* the patient was again examined, in conjunction with Mr Duncan, and on this occasion the second bicuspid and first molar on the affected side were found to be loosened, whereas four days previously they were quite normal. Mr Duncan after this opened the antrum through the alveolar process, and let out a quantity of fœtid pus. The case is still under treatment, but under continued irrigation, with boracic lotion and free drainage, is sure to do well. I have detailed this case at length, in order to illustrate the fact that the absence of bad teeth is no guarantee against antral mischief. As I have before said, antral empyema is commonly unilateral. To make this paper complete, however, it is necessary to state that in a few cases Ziem found the affection to exist on both sides. So far I have not met with such a case, but the diagnosis could, no doubt, be arrived at without difficulty.

In conclusion, it remains to refer briefly to the treatment. I do not think that Stoerk's suggestion to treat these cases by the introduction of a tube into, or, if this be impossible, up to the natural opening between the antrum and the nose, and thus to irrigate the cavity, is at all advantageous. Manifestly this line of treatment, even if in the end successful, must entail so much time and trouble as to weary the average patient. No doubt the establishment of a counter-opening is more scientific and satisfactory. Such a counter-opening may be made through the socket of a tooth, through the alveolar process, or finally through the outer wall of the nose near its floor, as suggested recently by Mikulicz. After a free drain has been established, irrigation of the antrum must be undertaken, and continued until the case is cured. For injection, such fluids as boracic lotion, or water containing small quantities of iodine or chloride of zinc, may be used, according to the indications afforded by each particular case. Whether the insufflation of powders in addition (boracic acid, iodoform, etc.) would prove more rapidly effectual I am not yet prepared to say.

The President said he could corroborate much of what had been communicated by Dr M'Bride. In many of these cases there was no pain, and the patient had no suspicion of antral disease. Sometimes the frontal, ethmoidal, and the sphenoidal sinuses might contribute as a means of keeping up the nasal discharge, but none of the sinuses were subject to the same irritation or liability to morbid conditions as the antrum. He believed that when carious or loose, or in any way suspicious teeth existed, with symptoms of an ozænic tendency, they should be extracted. The antrum retained its secretion much more than the other sinuses. Its floor was really lower than the palate, and was deep behind the posterior bicuspid, while the opening into the meatus was near its roof. In this way he had no doubt that in these antral cases, by its retention the mucus or muco-pus became very offensive. When these cases required opening, he thought it was best done above the external alveolar process. Such an opening closed much easier than one made through the socket of a tooth which often healed with difficulty, a sinus or fistulous opening continuing long after it was necessary. Opening the antrum was not required in every case. Where no obstruction or distension existed, he had seen removal of the offending teeth followed by disappearance of all the symptoms. In some jaws the antrum was not easily opened from the outside.

Professor Annandale considered that the most important and practical point of Dr M'Bride's paper was that such cases were often not discovered. He was quite willing to admit that improved rhinoscopic examinations might be an assistance in their diagnosis, but he was inclined to think, from his surgical experience, that there were always local symptoms which pointed to the presence of pus in the antral cavity. His experience was that if there was a discharge from one nasal cavity, the antrum on that side should be carefully examined, and invariably he had found that a practical surgeon accustomed to examine carefully would be able to find some tenderness or slight enlargement, showing something wrong with the antrum. The last case he saw was that of a young gentleman, who had undergone a variety of treatment for a nasal discharge without curative result. He found slight tenderness, and opened into the antrum, and was rewarded by finding pus. He was strongly of opinion that the best treatment was to open into the cavity above the alveolar margin. If there were diseased or loose teeth, the fangs of which were causing irritation, it was well to remove them, but his experience was that the opening above gave better and quicker results as regards cure. He found a considerable difference in regard to the size and even the position of the antrum. In some cases he had had to puncture very near to the floor of the orbit to effect an opening.

Dr Horsley asked if Dr M'Bride had any experience as to which of the various openings recommended, through the socket of a tooth, the external alveolar process, or through the nose, as practised by the German surgeons, gave the best result?

Dr Cotterill believed that the best plan was to attack the condition from the outside, the alveolar, not the nasal surface. The opening into the antrum would be on too high a level if made from the inside. Some cases treated by the internal opening were reported as cures in some of the medical journals a short time ago, but without sufficient details to enable one to judge. In the last case he had operated on, there was no enlargement and only slight tenderness on pressure. He made the opening through the canine fossa. The teeth had been removed sometime before, and the sockets filled up, and an opening through them would have been more difficult. He had the cavity syringed out twice a day through a glass drainage tube covered with rubber at the end. This was done for a fortnight till pus ceased to appear.

Five days after removal of the tube the cavity had filled up again. It was reopened, the tube reinserted, and left in for about three weeks with a successful result.

Dr Aitken mentioned a case in which there was no difficulty about the diagnosis. The pus had made its way through the hard palate, and gave rise to two swellings in the roof of the mouth. Behind these swellings was a small ivory exostosis. The first attempt to open into the cavity was made by an endeavour to extract a stump, thought to be the canine, but owing to the pain, and to the fact that the antral wall was so thin, the antrum was opened above the alveolar margin. Immediately that was done the stump dropped out, showing the considerable part the atmospheric pressure plays in maintaining the teeth in their sockets. A drainage tube was kept in for about ten days, and the cavity was washed out with an antiseptic solution introduced by means of a Eustachian catheter attached to a syringe. The case was mentioned to show the great importance of free drainage and perfect asepticism.

Dr M'Bride thought Professor Annandale was inclined to criticise the value of the rhinoscope in those cases on the ground that the experienced surgeon would find external evidence of the presence of pus in the antrum. But he had shown that in them there need be no bulging, no swelling, no pain, no tension, though there might be some congestion of the gum, and in all but one of his cases there had been diseased teeth. Unilateral discharge was not sufficient evidence of antral affection, inasmuch as it might be due to syphilis, rhinoliths, foreign bodies, or other causes. He thought that in many cases the arch of the palate might be taken as a guide to the size and position of the antrum.

The President said the antrum varied very much in its capacity, shape, and position in different subjects and at different periods of life. Where the arch of the palate was high it was very much more out of reach. It could not well be opened through the socket of the canine tooth, as its limit anteriorly was about the second bicuspid. It did not seem, however, to be what might be called antral teeth alone that were connected with this affection, as removal of diseased incisors had been of marked benefit in some cases, probably by the removal of diffuse irritation throughout the superior maxilla.

Mr A. G. Miller said he had once suffered from an acute abscess of the right antrum. He got great relief from the use of warm fomentations, and found that when he lay on his left side he was relieved of pain, and the matter trickled away slowly and intermittently. He cured himself by lying in bed in this position for two or three days. He thought that something of the kind might be tried in chronic cases along with antiseptic irrigation of the nares.

2. CLINICAL REMARKS UPON THE OPERATIVE SURGERY OF THE MALE BLADDER.

By Thomas Annandale, F.R.S.E., etc., Regius Professor of Clinical Surgery in the University.

I.—*Cystotomy in Certain Cases of Enlarged Prostate with persistent and aggravated Irritability of the Bladder.*

The cases referred to under this head are those in which all ordinary treatment, including careful catheterism, washing out the bladder, and general remedies, fail to relieve the almost constant distressing symptoms of pain, spasm, and desire to pass urine.

Various operative procedures have from time to time been suggested for the treatment of such symptoms, all of these having the object of establishing an artificial opening communicating with the bladder, and of allowing a metal or rubber tube to be inserted, so as to drain off the urine.

Sir H. Thompson, in the *Lancet* for 1875, vol. i., suggested that in certain of these cases the bladder should be punctured above the pubes, and that a tube should be permanently retained, and he described a special instrument for the purpose. Mr Thomas Smith, in St Bartholomew's Hospital Reports (1881), also advocates this proceeding. Mr Reginald Harrison, in *Lancet*, 1886, vol. i., advocates a perineal cystotomy with the introduction of a tube in this situation. Most surgeons are, I think, now of opinion that the perineal opening is the best in the large majority of cases in which this operation is required. Such is certainly my opinion, and my reasons are,—(1.) That the bladder is better drained from this opening; (2.) That the incision through the prostate sometimes has a beneficial effect upon the enlarged organ. Believing in the advantages of this proceeding in certain cases, my endeavour has been to improve the retained tube so as to permit of its permanent use, when required, in a form efficient and comfortable to the patient, and after several trials the apparatus now to be described appears to me to carry out these principles. An india-rubber catheter of full size having been cut short, so that its rounded end will lie in the bladder and its cut extremity project half an inch from the perineal wound, has fitted into the projecting end a short tube of hard vulcanite half an inch in length. This allows a silk or other thread with two short loops to be firmly fastened round the catheter without interfering with its canal, and by means of these loops the catheter is secured in position in the usual way. To the other end of the vulcanite tube any convenient length of india-rubber tubing can be fixed, and by placing a small stopcock on the tubing the bladder can be readily emptied at any time by turning on the tap.

In illustration of this treatment I give brief notes of the two following cases:—

Case I.—Mr S., æt. 70, was first seen in consultation with Dr J. Jamieson in July 1886, on account of prostatic retention, with cystitis and great irritability of the bladder. The patient being unable to empty his bladder, regular catheterism, with washing out the bladder and general treatment, were advised. This treatment was carefully carried out by Dr Jamieson, and I saw him occasionally in consultation. As the patient's symptoms were only temporarily removed, and as they became so much aggravated, I performed perineal cystotomy upon the 8th January 1887. The result was most satisfactory, and a few months after the operation he was able to go about wearing the tube which I have described. Up to this date (May 1888) he continues well, and Dr Jamieson writes me that "from a life of misery he has passed to one of comparative comfort."

Case II.—Mr —, æt. 60, was seen with Dr Dunsmure in April 1887. His symptoms were very similar to those affecting the patient in Case I. He had been confined to bed for several months, and various forms of treatment carefully tried without giving him relief. Cystotomy was performed upon the 9th of May 1887, and after a few weeks the tube, as described, was introduced. Since that date the patient has worn the tube and is able to go about and perform his duties.

The result obtained in these two cases is, I think, most encouraging, and it will, I hope, assist in establishing this operation as an efficient means of relief in suitable cases.

Very recently it has been suggested by Mr M'Gill that a special cystotomy may be employed for the removal of a portion or portions of an enlarged prostate with a view of taking away an obstruction to the natural passage of the urine. It has been long known that projecting portions of an enlarged prostate may be safely removed. More than twenty-five years ago, when assisting the late Mr Syme to perform lateral lithotomy upon an old gentleman, a large portion of the prostate was accidentally caught between the blades of the forceps and torn away. This patient made an excellent recovery. Similar cases have been recorded, and I think that when an exploratory incision has demonstrated a projecting enlargement of the prostate into the cavity of the bladder which is interfering with proper micturition, the removal of this portion is a proper and justifiable proceeding.

II.—*Cystotomy in Certain Cases of Acute Retention of Urine, the Result of Enlarged Prostate.*

When acute retention of urine takes place in patients suffering from enlargement of the prostate, it may be temporarily and easily relieved, more especially if it is early and carefully treated, but the condition is not unfrequently tedious in its progress and complicated by cystitis and general irritative disturbance and fever more particularly when treatment has been delayed or unsuccessfully employed. When the required use of the catheter irritates, or when its employment is unsatisfactory owing to the difficulty of passing it into the bladder, to the want of experience on the part of the medical attendant, or to the circumstance that the patient is distant from proper surgical aid, I am of opinion that a perineal cystotomy is the most likely means to give the patient efficient relief. No doubt in urgent cases supra-pubic aspiration or puncture is a valuable means of relieving the retention; but this proceeding can only be temporary, even if frequently repeated, and experience of it has shown that it is not without risks. On several occasions, when called to distant parts of the country to consult in regard to difficult cases of prostatic retention, I have advised and successfully performed perineal cystotomy, believing that this treatment was best for the comfort of both patient and his medical attendant.

III.—*Cystotomy for the Removal of Tumours of the Bladder.*

The experience of this operation has already proved that in favourable cases—that is, in cases of tumours having little or no malignant tendencies—encouraging results are obtained. It cannot yet be said that the exact diagnosis of the presence of a tumour in the bladder can always be made, nor can the exact nature of the tumour, if present, be always determined by mere symptoms, microscopic examination of the urine, or external examination, but an exploratory incision will in all cases of doubt decide the question.

From my experience in connexion with operations for the removal of bladder tumours I would wish to express an opinion as to the best form of cystotomy in such cases. I have no hesitation in saying that tumours of the bladder can be most accurately and thoroughly removed by making both a supra-pubic and perineal incision. If the forceps or other extracting instrument is introduced through the perineal wound and their blades guided by one or more fingers passed into the bladder through the supra-pubic wound, an accurate removal of the growth is, in my opinion, most thoroughly obtained, more particularly in the case of broad-based tumours. It is quite possible to remove a tumour of the bladder through either a perineal or supra-pubic opening, but I believe that by means of the two openings a greater accuracy of removal is possible.

My usual practice is to first make a supra-pubic cystotomy, and then a central perineal one, in all cases in which the diagnosis of the presence of a removable tumour is well determined; but when the diagnosis is doubtful, I first make a central perineal exploratory incision, and then, if required, follow this up by a supra-pubic cystotomy.

IV.—*Cystotomy for persistent Irritability of the Bladder unrelieved by ordinary careful Treatment.*

Cases of this nature are not uncommon, and they may or may not be associated with symptoms of cystitis. When all ordinary treatment fails to relieve, perineal cystotomy gives, in my opinion, the best chance of curing or relieving the condition, and it is from my experience much more effective than any drainage of the bladder by the retention of a catheter or tube passed along the urethra. In some of these cases little or no change in the bladder structures can be detected; but in others I have found superficial ulceration or a velvety condition of the mucous membrane. Needless to say that before practising cystotomy in any case of this kind all sources which are known to produce symptoms of irritability of the bladder should be carefully investigated.

Note.—The recent suggestion that the endoscope may be employed by passing it through a perineal wound, such as is made in perineal cystotomy, is, in my opinion, a most valuable one, and is likely to much assist the correct diagnosis of obscure bladder conditions.

Dr Hodsdon asked if urine trickled along the outside of the tube when it was fixed in the bladder, how often the tube required changing, and if the patient could do it himself?

Mr A. G. Miller thought the operation was not done as often as it might be in cases of enlarged prostate and septic cystitis. It was not difficult nor was it dangerous. It was simply carrying out in the most efficient way the surgical principle of draining at the most dependent part. He had at present a patient under his care suffering from septic cystitis with dilated and sacculated bladder. He made up his mind to perform perineal section, but when about to do it saw the use of thymol recommended in such cases. He began the thymol internally six weeks ago, and since he began he had not once required to touch the patient's bladder, though previously it had to be washed out twice a week.

Dr Cotterill asked if Mr Annandale incised the membranous or prostatic portion of the urethra. Mr Harrison, he thought, had insisted on the necessity of opening the membranous portion and dilating the prostatic part so as to preserve the sphincter action of the prostate, and prevent the leakage to which Dr Hodsdon had referred?

Dr Symington suggested that if the wound possessed sufficient contractility to prevent urine trickling down the sides of the tube it might also compress the tube itself, so as to prevent the flow of urine until the patient attempted to pass water, when the pressure would dilate it.

Professor Annandale replied, that a certain amount of urine did at first pass down the outside of the tube, but he found that after the ordinary lithotomy tube had been in for about ten days and an indiarubber one inserted, it did not tend to pass. The changing of the tube depended on the individual case. Sometimes it required to be changed every fourth or fifth day, sometimes every tenth. The patient could reintroduce it himself if he did not leave it out too long. He had had no experience of thymol. As to the part of the urethra opened into, he did not think one could always be accurate, but he endeavoured to strike as close to the prostatic portion as possible. He rather thought that Mr Harrison did advise and perform incision of the prostate. They differed as to the direction—Mr Harrison performing it laterally, while he (Mr Annandale) performed it upwards. He was of opinion that the incision was of advantage, inasmuch as it led to a diminution in size of the prostate gland.

Special Meeting.—February 15, 1888.

Dr John Smith, *President, in the Chair.*

I. Exhibition of Patient.

Dr Felkin showed a patient of his who had been trephined by Mr Hare over the MOTOR AREAS OF THE BRAIN for localized paralysis. There was commencing return of the lost functions. The patient was a girl aged 17, who had received a fracture of the skull when ten months old. The right arm and leg were almost completely paralyzed; they were shorter than the left arm and leg, and badly developed. The temperature was 2° lower than on the left side; the reflexes were exaggerated, and sense of locality and tactile sensation were absent. At the operation, which was performed a month ago, a large cyst, which extended to a depth of two inches from the surface of the skull, was found, and also an osteophytic growth, which extended inwards half an inch towards the surface of the brain. The patient made a good recovery. She walks better, can move her arm to a considerable extent, and both reflexes and temperature now correspond on both sides of the body. The case will be published in full at some future date.

II. Original Communication.

ANIMAL TUBERCULOSIS IN RELATION TO CONSUMPTION IN MAN.

By Thomas Walley, M.R.C.V.S., Principal of the Edinburgh Veterinary College.

Mr President and Gentlemen,—Allow me, before entering upon the subject matter of my paper, to express my sense of the honour you have done me in according to me permission to bring before you for discussion this evening a subject which to my mind is the most important of all the subjects connected with comparative pathology, and you will perhaps pardon me if I say at the outset that it is a subject which has been most strangely neglected and most severely ignored by the great bulk of the members of the medical profession in this country. Indeed, with the exception of Drs Creighton, Woodhead, and Bland Sutton, I do not know of any medical men who have devoted any large amount of energy or of time to the elucidation of a problem which has a more important bearing on the health of the human race than has any other problem at present claiming the attention of the profession. In spite of all the attempts which have been made in the past to minimize the importance of animal consumption in its relation to this fell destroyer of the human race, the fact still remains that in every essential particular the tuberculosis of man and the tuberculosis of animals are identical. The scourge of the bovine species is the curse of the human, as it is also the pest of the feathered tribes.

A few years ago, comparatively, animal tuberculosis received but a scanty share of attention from the members of the veterinary profession in this country, and few in its ranks associated the malady with that most fatal and most intractable of all the diseases to which human flesh is heir; and, whether from the supineness exhibited by the members of the sister profession on this subject, or from the growing enlightenment of the members of the veterinary profession, I cannot say, but assuredly the latter have within the last twenty years taken the most prominent part in bringing the subject of tuberculosis under review.

In glancing for a brief space at the history of the disease, I need scarcely remind you, I think, of the fact that it is almost coexistent with the science of medicine; and probably, if we could look into the ages that were, we should find that its genesis was coeval with that of man.

If it is a fact that consumption in man has always held a prominent place in the literature of the medical profession since medicine became an art, it is equally a fact that the disease has been recognised by various cognomens among veterinarians for generations and in different countries as being common to animals.

But while English veterinary practitioners and others spoke glibly of "grapes" in cows, or, referring to a well-known symptom (diarrhœa) as "shooters," and to another symptom (sub-maxillary œdema) as "wattles," and while the characteristic growths of scrofula and tubercle were in many of the northern counties of England and in some of the Scottish counties designated and known as "clyers" and "wens;" and further, while the disease was known as "angleberries," and the affected animals themselves spoken of as "piners" or "wasters," still no systematic inquiry into its nature was made by those responsible for veterinary teaching until within the last quarter of a century; in proof of this I may quote the fact, that during his admirable course of lectures—the most complete of any that were delivered at that time—Professor Simonds of the London Veterinary College did not once refer to the disease during my pupilage in the years 1861-3; and in 1847 there occurs in the professional organ of that day, the *Veterinary Record*, the following remarkable statements in connexion with some comments on what was evidently a case of nodular tuberculosis, which had been brought to the notice of the editor by a practitioner, Mr W. Cox of Ashbourne. The statements are:—"The tumours attached to the viscera of the thorax of a cow are similar to some we have occasionally met with;" and, "On this subject our English veterinary authors are silent, and we believe the above to be the only case of the kind recorded."

Personally, I became acquainted with the affection as "grapes" in the early days of my professional pupilage, and, thanks to the enlightened ideas entertained as to its nature by my preceptor Mr Kettle, I was also familiar with it as "consumption."

In 1872 I had the privilege of reading a paper on this subject at a meeting of the West of Scotland Veterinary Medical Association, which was held in Glasgow; and again, in 1879, I included the disease in the book entitled, *The Four Bovine Scourges.* On various occasions since the issue of that book I have brought the matter before the profession. During the period mentioned, other members of my profession have written upon and publicly noticed this subject, and for some years there has been, practically, unanimity amongst us as to the danger of transmission of the affection from animals to man. On the Continent the dangerous nature of tuberculosis was recognised at a very much earlier period than here, and we are told (*vide* Lydtin, Fleming, and Van Hertsen, on "The Influence of Heredity and Contagion in the Propagation of Tuberculosis") that in the Mosaic laws special provision was made for the condemnation of the flesh of animals suffering in the advanced stages of phthisis.

In addition to the doubts which have existed in men's minds as to the relation of animal and human tuberculosis, we may notice the difficulties surrounding its pathology. It is amusing and interesting to revert to the ideas entertained by different authors on this point, and to remember that such epithets as "miserable abortion" and "degraded exudate" were freely applied to tubercular lesions. Bayle and Laannec were probably amongst the earliest to recognise its true nature, histologically and etiologically, and I must confess that the published opinions of the latter exercised a powerful influence on my mind from the date of my earliest pathological acquaintance with the disease.

The promulgation of the opinion by Laannec that the existence of tubercles was always associated with a caseous centre in some organ or other of the body, was a vast stride towards the realization of its exact causal relationship. Then came the declarations of Villemin, founded on extended experimental inquiry as to its infectious character; and following on this, I think I am correct in saying, that the doubt cast upon the nature of the disease, in animals at least, by the use of the term lympho-sarcoma by Virchow, was the means of temporarily arresting the progress of the realization of an exact knowledge of its pathology; and, I may say also, of checking the realization of the intimate relationship existing between the tuberculosis of man and animals.

In Cohnheim's celebrated dictum, "That that which is of tubercular origin can alone originate tubercle," we had sounded the key-note of a great discovery; but it again was obscured to some extent by the declaration of Sanderson and others, that tuberculosis could be produced by inoculation with many and diversified pathological products. It remained for Koch to place the crown upon the edifice of our knowledge as to the specific nature of tuberculosis, though even his grand work had been foreshadowed to some extent by other inquirers. So far as I am personally concerned, I arrived at the conclusion at an early period of my professional career, *that tuberculosis was a specific disease;* and in the essay prepared in 1872 occurs the following remark, "Tubercle is as much a specific disease as is syphilis, or any other affection depending for its continuance (? existence) in the system upon a morbid poison or principle." Again, in the *Four Bovine Scourges*, p. 150, "In whatever way I consider the character of tubercle, as tubercle, my mind always reverts to the same conclusion, viz., that tubercle is as much a specific disease, whether inherited or acquired, as glanders, syphilis, or any other affection of the kind with which we are acquainted."

The truism in reference to new inventions, to the effect "that, considering their simple character, it is a wonder that some person had not thought of them before," is equally a truism in reference to pathological discoveries. Long before Koch's discovery, or rather his recognition of the tubercle bacillus, veterinary surgeons were acquainted with the fact that the tubercular nodules, so universally met with in the lungs of sheep, were simply a pathological expression of the effects of a parasitical irritant in the shape of the embryo of the Strongylus filaria; and it would have only been a logical sequence had some one who had carefully considered the matter arrived at the conclusion that the cause of tubercle as tubercle was an irritant; that that irritant was an organized entity in the shape of a bacillus, or that the irritant had some connexion with such an organism—a secretion or product thereof, perhaps; that the irritation so produced established a localized exudative inflammation followed by a formative process, which, however, only attained to a certain degree of perfection; and, in consequence of the continued irritation and of the imperfect nature of the cell

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structures produced, that it was followed by a retrograde change which culminated in the recognisable caseous and calcareous products with which we are all so familiar.

Not only would such a line of reasoning have led us to the discovery of the true nature of tuberculosis, but it would have shed a flood of light upon much that was obscure in reference to the nature of glanders, actinomycosis, and other forms of that group of diseases now classified as the infective granulomata. Moreover, it would have been the means of leading us to discard, at an earlier period than we did, the, to us, distinctive terms "gray," "yellow," and "infiltrated" tubercle,—terms which of themselves signified only the features presented at different stages of its growth of one and the same product. And even if it had not justified us in preserving a strict adherence to the expression used by myself in the *Four Bovine Scourges*, to the effect "that tubercle is a visible local manifestation of a constitutional diathesis (scrofulosis)," it would at least have led to the inquiry, What is the essence of scrofula?

In order that the members of this Society may the more readily realize the intimate connexion which exists between animal and human tuberculosis, it is advisable that I should consider at the outset some questions of a general nature, such as the species of animals most liable to tubercle, the means by which it is propagated, the organs affected, its course, its effects on the system, its macroscopical characters, and its complications.

(1.) *Species of Animals affected.*—It is universally acknowledged that the members of the bovine species are pre-eminently the hosts of tubercle. In them it has been thought to be indigenous, though why bovines should be more predisposed than other animals is not easy of explanation—except perhaps the fact that their complicated stomach may afford a resting-place for the bacilli, or that there may be some element in the bovine constitution favourable to their support, in other words, that the doctrine of *receptivity* applies in this case more powerfully than in that of animals of other species. Looking at the fact that the sheep bites more closely to the ground than does the ox we should be inclined to think that ovines stood a greater risk of ingesting the viruliferous principle with their food than do bovines, but it is a most remarkable fact that the former are seldom found to be the subject of naturally contracted or spontaneous tuberculosis. Indeed, I may say that of all the domesticated or semi-domesticated animals whose flesh is used for human food the sheep stands out from the group as the one least liable to tubercle; and this is all the more remarkable seeing that this animal is so intimately associated in many cases with cattle and with rabbits, a creature pronouncedly predisposed to and very largely the subject of tuberculosis. Equine tuberculosis of natural origin is comparatively rare,—so much so, indeed, as to lead me to make the remark in the course of my lectures (at least up to a comparatively recent date), that notwithstanding the fact that in 1864 I placed on record what I conceived to be a case of tuberculosis in this animal, I had subsequently to confess that I was mistaken. The pulmonary nodules of glanders, in their macroscopical and histological aspects, bear the closest analogy to those of tuberculosis, and in its clinical aspects glanders has as many analogies to tuberculosis as it has to syphilis; but the disease of all others that in its general characters, method of dissemination, and course most closely resembles tuberculosis, is that which we have been in the habit of describing as lymphadenoma. From a consideration of its clinical characters, its course, and its method of infection as between different parts and organs of the body, I was led to make the following declaration at p. 165 of the *Four Bovine Scourges*, "Lymphadenoma bears, in its history, in its method of dissemination from pre-existing centres, and in its clinical characters, the closest analogy to tubercle of any morbid product with which I am acquainted; in the particulars mentioned it is the tuberculosis of the horse." "It differs, however, from tubercle in the fact that it is occasionally seen in all animals, though resembling it in most frequently choosing for its victims certain species (as the equine and canine), in which it may commence in the lymphatic glands and extend thence, by a process of auto-inoculation, to almost every tissue in the body, exactly in the same way as tubercle does in acute tubercular infection; and when so disseminated the secondary nodules are often very small in size."

"The important distinctive characteristics of lymphadenoma are, that its lesions are nearly always found in the spleen; that its nodules are of a white or grayish-white colour, sometimes soft and brain-like in consistence, at others firm; and in the latter case large capillary vessels are seen radiating from the centre of the nodules."

To these remarks I might have added, that the clinical characters of some of the cases which have come under my observation establish even a more close identity. Thus, I have seen the arachnoid membrane studded all over with submiliary nodules in the case of a horse I had had under observation for some time as the subject of lymphadenoma, and in whom symptoms of cerebral meningitis made their appearance prior to death; in another case I found, during life, certain lesions of the buccal membrane which, coupled with enteric symptoms, might well have been those of tuberculosis.

Koch's discovery of the tubercular bacillus has, as I have before remarked, given us a more accurate basis upon which to found a diagnosis; and several Continental pathologists, notably Johne, have within the past few years discovered its existence in the lesions of so-called lymphadenoma; while, during the past year, my colleague, Prof. M'Fadyean, demonstrated the bacilli in what appeared to be lymphadenomatous nodules in the spleen of the horse.

Another morbid product in the horse which in its general characters bears a close resemblance to tuberculosis is cerebral psammoma. In the early stages of its growth, the choroid plexuses present a condition strikingly like that of the vascular villi of the pleura and peritoneum in acute tuberculosis of those structures; and in the advanced stages, the groups of gray or yellow nodules can be scarcely distinguished by the unaided eye from those which characterize this disease.

In reference to the occurrence of equine tuberculosis, Gerlach made the following important remark, "The fibrous tubercle of horses, destitute of cheesy matter, is as infective as the tubercle of cattle, though *real* tuberculosis is rare in this animal."

In the *Four Bovine Scourges* I penned the statement, that "the pig was very prone to tubercle." That assertion was based on a wide experience, which has been strengthened by closer investigation in recent years; but it is a curious fact, that one well-known Continental veterinary pathologist some time ago declared that the pig was not subject to spontaneous tubercle, and a similar opinion was also recently expressed by one of the examiners of the Veterinary Examining Board of this country.

If we except some of the lympho-sarcomatous formations we sometimes meet with in the lymphatic glands, and those nodular productions occasionally met with in the spleen, we are justified in saying that nothing resembling spontaneous tuberculosis is ever seen in the dog. The system of this animal possesses a happy immunity from the ravages of tubercle; and the same remark applies to the cat, though the disease has been artificially or experimentally induced in these animals with great readiness by a number of experimentalists.

Of poultry, tuberculosis claims a holocaust of victims, and macroscopically it presents in these animals some remarkable divergences from the recognised characters of tubercular products in the ox and in man. My first intimate acquaintance with avian tuberculosis was about the year 1865; but several years prior to that date I had seen cases which to the naked eye presented features closely analogous to the disease, and that more particularly in the rook. In subsequent years, I am free to confess, my ideas on the subject became rather confused, mainly from the fact, that it became the fashion amongst our Continental confrères to designate such cases by the term "Gregarinosis."

From the earliest period at which I lectured to my class on this subject, I pointed out that the difficulty in recognising the disease in poultry lay in the fact, that the resulting lesions were of a totally different character than were those found in the organs of other animals; and this remark more particularly applied to the intestinal, pulmonary, and cutaneo-cellular lesions. I was also, at a very early period of my acquaintance with the disease in the feathered tribe, struck by the fact that water-fowl—as ducks and geese—were peculiarly exempt from its ravages. In an admirable article on this subject in the *Journal of Comp. Med. and Surgery* for October 1886, Dr Bland Sutton points out that, in his experience, grain-feeding birds become most largely the subject of tuberculosis; and also that carnivorous birds sometimes fall victims to it, from devouring the dead bodies of other creatures.

Amongst the semi-domesticated animals the rabbit takes the foremost place as the host of tuberculosis.

As a boy I was accustomed to see myriads of tubercular nodules in the livers of rabbits, and so much was I impressed by the fact, that in the essay already alluded to as having been read by me in 1872, I suggested that there was a possibility of contamination of pastures by the fæcal matter of these animals. Guinea-pigs are especially predisposed to tuberculosis—at least by artificial infection—and the same may be said of monkeys, in which animals the lesions approximate perhaps more closely to those seen in the human subject than they do in any other animal. The first case I saw in this creature was brought to my notice by a late and respected colleague, Dr Davidson.

Amongst wild animals the disease is probably but little seen, at least so long as they are allowed to remain in a state of nature; but when placed in confinement they contract the affection equally as readily as do other animals, and we are indebted to Dr Bland Sutton for a record of its existence in the python. Probably there is no animal whose system is capable of withstanding the infection of tubercular virus; probably there is no disease that can with more appropriateness be distinguished as a panzöotic affection.

The propagation of tuberculosis is effected in a variety of ways,—by congenital and hereditary transmission, by ingestion of contaminated foods or water, *i.e.*, naturally or experimentally; by natural or experimental inhalation, by accidental and intentional inoculation, and by intra-venous injection.

The possibility of congenital transmission of tuberculosis occurring was for a long period denied, but there are those amongst human pathologists who believe in such a possibility, and who assert that it does occur, while veterinary pathologists have at different times adduced practical facts in proof of such a possibility. It has not as yet fallen to my lot to witness congenital tuberculosis, but I have seen it in animals at such an early age as to lead me, perforce, to the conclusion that it must have had an *intrauterine* origin.

Mr M'Gillivray of Banff has put on record evidences of the fact that congenital transmission takes place, and some six years ago I received a communication from a late pupil of my own, Mr Frank Ashley, giving me details of some cases in a valuable herd of shorthorns, which were under the care of Mr Carter of Guildford, and in which young calves developed symptoms of tubercular meningitis and pulmonary tuberculosis within a few days after birth. Why doubt as to the probability of such transmission taking place should be entertained in view of our knowledge of the existence of tubercular orchitis and öophoritis in animals, and of the existence of bacilli in the blood, with the ready transmission of the anthrax bacillus (a much larger bacillus than that of tubercle) from the mother to the fœtus, I cannot understand. Several years ago a lady of my acquaintance purchased a setting of eggs from a poultry-yard which I knew was infected, and shortly after the birth of the chickens they showed evidence of the existence of the disease, and very quickly the other poultry on the premises became affected.

As to hereditary predisposition, the fashion at the present day in human medicine seems to be to explain the fact by assuming that the systems of certain individuals—on account of family peculiarities—offer a more favourable soil for the development of the bacillus than do the systems of others. This hypothesis, to my mind, is a convenient way of explaining an unpleasant phenomenon, viz., that family after family is swept off the face of the earth by this fell malady; but I confess I cannot understand why such a theory should be accepted in face of the many and palpable proofs to the contrary one meets with in one's intercourse with individual families.

I am well aware that the question—a most difficult one to answer—may be asked, What becomes of the virus during the years that intervene between the birth of individuals and the development of the visible manifestations of tubercular lesions in their systems? You acknowledge the possibility of syphilitic lesions being developed in children in whose systems a hereditary taint exists; is the causal entity so widely different in these diseases as to preclude the possibility of hereditary transmission in the one if you acknowledge that it can take place in the other? I grant it is difficult to understand in what particular tissue or organ the bacilli or their spores reside in a quiescent manner for prolonged periods, but when we observe the same process going on in the case of the virus of rabies, the difficulty is not so great. In the calf I have seen scrofulous lesions in the thymus gland, and scrofulous processes are common in the lymphatic glands of these animals as they are in human juveniles. May not the bacilli or their spores be imprisoned in the structure of these glands, and retain their vitality and power of reproduction until some circumstance favourable to their admission to the general system arises?

Propagation by Ingestion has for long, thanks to the united labours of human and veterinary pathologists, been an established fact. Not only has the disease been propagated by the ingestion of its products, but also by the ingestion of the secretions of affected animals; and not only this, but it has been shown that a tolerably high degree of heat does not always insure the inocuity of such an important animal product as milk. This method of propagation, it has been abundantly proved, is effected between animals of different species, as from the cow to the pig, and from the former animal to the common fowl; and in addition to this, we have recently had several strong statements made as to the probability of human infection by the ingestion of improperly cooked animal food, especially that of poultry. I have already alluded to the suggestion thrown out by myself in 1872, to the effect that cattle may become infected through the medium of pastures contaminated by the fæces of tuberculous rabbits, and an observation has been made by several veterinary surgeons and by stock-owners to the effect, that every animal confined in a particular and contaminated stall in particular byres became sooner or later a victim to tuberculosis. My late respected friend, Mr Dewar of Midmar, had observed this as long ago as the year 1839. Grad and others have placed on record abundant evidence of the occurrence of such a method of infection, and only a few months ago a Wigtownshire farmer in the course of a conversation with him on the subject mentioned to me a striking example of the same contingency in his own stock. By some it has been thought that infection takes place through the medium of the lungs in these cases, but the majority of observers agree in attributing it to the ingestion, by licking (a habit common to cattle) of the dried sputa from the manger or other surroundings of infected occupants of the stalls, or by the ingestion of contaminated foods. Reference to this matter leads me to offer a remark on a very misleading statement made by human pathologists, and made by one medical man very recently (Dr Handford in the *Lancet*), to the effect that in pulmonary phthisis of animals there is no sputum. As I pointed out in a subsequent issue of the *Lancet*, this statement is one of many exposing lamentable ignorance on the part of many medical men as to the clinical phenomena of disease in animals.

Granted that animals do not eject sputa at will, and in such unnecessary profusion as is seen too often to be the case in man, the ox and the dog do on occasion readily expectorate; and independently of this, the materials which in man would be got rid of by expectoration are passed out of the bronchial tubes by the nose, or after being coughed up into the fauces are swallowed, and ultimately passed out with the fæces to lie upon and contaminate the food, the litter, or, in the case of cattle, the pastures.

In the matter of the propagation of tubercle by the medium of the drinking water, if we acknowledge that it may be propagated by the ingestion of solid, there need be little difficulty in accepting the dictum that it may be propagated also through the medium of fluid ingesta.

At a meeting of the National Veterinary Association held in London in 1883, Mr Olver, F.R.C.V.S., of Tamworth, expressed an opinion to the effect that a number of cattle in his neighbourhood contracted tubercle by the ingestion of sewage matter from the town.

I have myself seen one instance in which the circumstances connected with the death of a number of young cattle pointed to infection by contamination of the drinking water with sewage; and in March 1886 I received from Mr Menzies, M.R.C.V.S., of St Austells, sections of the lungs of an ox affected with tuberculosis, and at the same time a communication from that gentleman, in which he stated that not only the feeding bullocks, but the milch cows also on a particular farm in his neighbourhood were found on examination to be the subjects of pulmonary tuberculosis; the water supply in this case was constantly contaminated by the liquid filth from the farmyard and other places. Independently of contamination by sewage matter, both drinking water and food may become fouled by the urine and the uterine discharges of infected animals.

Propagation by Inhalation has passed out of the region of speculation; it is now shown that, experimentally at least, its accomplishment is certain. I do not myself see that there need be any question as to such a probability, seeing that the expired air from the lungs of tuberculous animals as well as from the lungs of man must often be laden with bacilli or their spores from the softened products of broncho-pneumonic vomicæ; but independently of this, there can be but little doubt that particles of dried sputa and other contaminated animal products are liberated by friction and other forces into the atmosphere and become inhaled by animals. About the year 1869 I investigated an outbreak of tuberculosis in which a large proportion of the cows in a byre became affected and died, or were sent to the knackers.

Infection by inoculation of various products from the bodies of infected into those of healthy animals has been abundantly proved by numerous experimenters, more especially since the publication of the result of Villemin's labours; and the same may be said of subcutaneous injections, injections into cavities, and intra-venous injections; tubercular products in all stages of development and retrogression, the juice of the muscles, milk, urine, saliva, and blood have each and all been used in the successful transmission of the disease; and one experimenter (Toussaint) tells us he has succeeded in transmitting it by using the fluid developed in the vesicles of vaccinated tuberculous calves.

Probably there is no more interesting or instructive subject in connexion with the malady under consideration than the transmission, re-transmission, and inter-transmission of the malady by the means above noticed. From man to nearly every species of animal, from one animal to another of the same species, and from animals of one species to others very differently constituted has the interchange been successfully carried out; and if the resulting lesions have not always borne an absolute resemblance to their prototypes, they in this respect only stand on the same level as do the remarkable divergences in the lineaments of the countenances of closely related men and women.

The most important part, however, of this branch of experimental inquiry seems to me to lie in the probability or otherwise of the secretions from organs (healthy in themselves) becoming contaminated by the virus in the case of animals suffering from tubercular disease in other organs of the body. When general, *i.e.*, blood infection has taken place one can see that there exists more than a probability that the secretions generally will become contaminated—though this is, in my opinion at least, by no means necessary. Saliva is sometimes infective, but I have yet to see the first case of tubercular disease of any of the salivary glands. Milk is frequently infective, but I entertain (very strongly) the impression that it only becomes so when the udder is the seat of tuberculous lesions; but to this question I shall revert hereafter.

Organs and Structures affected in Spontaneous Tubercle.—In the ox it may with truth be said that the lungs, the liver, and the serous membranes are most largely the seat of tubercular lesions, and in reference to the abdominal and thoracic serosæ, it is thought that they are sometimes primarily affected; but while I do not deny the probability of this occurring, I think if a careful search were made some pre-existing visceral lymphadenous focus would, in the great majority of cases at least, be found. That other organs may become secondarily affected to the serosæ is highly probable, and it seems to be an accepted fact by some writers that peritoneal tuberculosis sometimes extends to the uterus *via* the Fallopian tubes; indeed, some make the statement that this is the only way by which uterine tuberculosis is induced. That this is not the case is proved by the fact that we frequently meet with cases of tubercular metritis in which no trace of peritoneal tuberculosis exists. From the pleuræ the disease often extends by auto-inoculation to the pericardium and endocardium.

Meningeal tuberculosis (either cerebral or spinal) is, in my opinion, always secondary in cattle, though there are some authorities who hold that it may be produced by inhalation *via* the nasal chambers and the ethmoidal cells. This, I think, is not probable.

While allowing that serous membranes become involved by continuity or contiguity, it is to me a remarkable fact that there seems to be but little or no tendency to extension therefrom to invested organs, or *vice versâ*. Pounds of tubercular products are formed in and upon the peri- and the epi-cardium, upon the pleura and upon the peritoneum, without a trace thereof existing in the organs they invest; and though I do not deny that eccentric extension may take place as from the bowels, liver, or uterus, to the peritoneum, or from the lungs to the pleuræ, I have no hesitation in asserting that this method of extension is, in animals at least, extremely rare. I have seen cases of cerebral with meningeal, and of spinal with meningeal tuberculosis, but it would be difficult to assign the order of precedence in either case.

Extension from the parietes or the parenchyma of organs to mucous membranes is very common. Thus we see bronchial secondary to pulmonary tuberculosis; but, curiously enough, I have never yet seen hepatic lesions involving the mucous lining of the biliary canals, though the reverse holds good in the case of the udder and the lacteal and galactopherous sinuses. In rare instances, except by ingestion or direct contact, do we see the superficial mucosa primarily involved—the lesion, as a rule, commencing in the submucosa, in which extensive lesions sometimes exist while the superficial mucosa is intact.

Extension to arteries from the surrounding structures is probably never seen,—at least, it is a phenomenon I have yet to become familiar with, except in the small arteries of the brain, and even here the peri-vascular lymphatic sheaths are more often the seat of the lesions than are the arterial walls.

Extension to veins takes place both in the case of the pulmonary and hepatic, and while I cannot give you a visible demonstration of the former fact (a pencil sketch I took some twelve years ago having gone amissing), I have brought for your inspection a section of liver showing the impingement of tuberculous nodules on the lumen of a large hepatic venous trunk. I have not as yet seen any tendency to irruptive processes in the case of the portal canals. Involvement of the pulmonary veins in the human subject is alluded to by several authors, but I have not noticed any reference to the hepatic. As by the former channel blood infection sometimes takes place, so it undoubtedly does by the latter.

Of the different glands of the body the lymphatics, especially in the ox and pig, hold pre-eminence in the frequency with which they are the seat of tuberculous lesions, and particularly does this apply to the bronchial and mediastinal groups. That these groups may become infected by inhalation, independently of the lungs, must, I think, be accepted without question; at any rate, we frequently meet with extensive lymphadenitis where no traces of lung lesions exist, and, as I have already indicated, scrofula of the laryngo-pharyngeal group of glands is commonly met with quite independently of systemic lesions; nay, more, veterinary practitioners in different parts of the country—notably in Cumberland and Westmoreland—aver that they frequently prevent systemic contamination by the early removal of one or more glands of this group. With hepatic tuberculosis we get involvement of the hepatic glands; while with mastitis the superficial inguinal group are simultaneously affected. There need, I think, be little doubt entertained on the point of systemic infection through the medium of the lymphatic glands. So long as the lesions are confined to the stroma of these organs there is not, probably, much danger of such a contingency arising; but in many instances there comes a time when the softening process breaks down all barriers, and the lymph vessels become the channels through which tuberculous products in abundance are carried into the general vascular circulation. I have seen the mediastinal vessels enlarged to the calibre of a crow's quill, and containing in their interior masses of caseous material as large as small peas. In like manner mesenteric tuberculosis, which is of comparatively common occurrence in cattle, becomes a medium by which infective matter is carried into the circulation *via* the thoracic duct.

The mammary glands, both of the cow and of the sow, are frequently the seat of tuberculosis, and while in the main the lesions are confined to the acini, irruption on the mucous membrane is of tolerably frequent occurrence, though sometimes difficult to trace by visual examination only. The pancreas is of all the glandular organs in the body the least frequently affected by tubercular processes. I have only seen it involved once or twice; and I have already remarked that, in my experience, the salivary glands enjoy a similar immunity.

Of the blood glands it may be said that while the thymus is occasionally affected, the thyroid bodies are seldom, if ever, involved; and it is a remarkable fact that the spleen is, in the ox, but very rarely the seat of tuberculous lesions, though it is more commonly affected in the pig, and, as before stated, some of the nodules we used to consider as a part of lymphadenoma in the spleen of the horse are of tubercular origin, and probably this is true also of the spleen of the dog.

Tubercular orchitis is frequently met with in the bull and in the boar, though I have not often seen it in male birds. The testicles of the ox and of the boar sometimes attain an enormous size; one testicle from the latter animal, sent to me some years ago by Mr Mitchell of Stranraer, weighed several pounds, and what is of vast importance in connexion with the congenital transmission of the disease, the vas deferens was enormously increased in calibre, its coats were thickened, and the mucous membrane so bestudded with tubercle nodules and ulcers as nearly to close its lumen.

Tubercular öophoritis is of common occurrence in cows; it is a frequent cause of nymphomania in these animals, and cows so afflicted are universally known as "bullers."

To *tubercular metritis* I have already alluded, and have remarked that the lesions seem most largely to have their origin in the submucosa in which we sometimes find comparatively large cavities containing caseous matter or pus. Localization of the lesions in this organ is a frequent cause of abortion in cows, while involvement of the cervix uteri produces one form of scirrhous os. *Tubercular vaginitis* is only very exceptionally seen; indeed, I may say that I have only met with one good instance of the lesion here.

Tubercular nephritis is also, comparatively speaking, common in cattle, the organs sometimes attaining an enormous size—one sent me by Mr Kettle, F.R.C.V.S., some years ago, weighing nine pounds; and in another case occurring in that gentleman's practice, the gland became disintegrated and discharged piecemeal by the ureter, the latter attaining a diameter of about 1½ inches. *Tubercular cystitis* is not often seen, and when it does exist the lesion is usually localized in the cervix; while *tubercular urethritis* is still more rare.

Passing to the *alimentary canal*, we find that spontaneous tuberculosis of the mouth, the tongue, the fauces, the pharynx, and œsophagus is extremely rare, except in young calves, in which it can be readily induced, at least in the mouth, fauces, and pharynx, by the ingestion of infective milk. In the ox the three first compartments of the stomach are never affected, and the true stomach but rarely. In the stomach of the pig it is more frequently seen, and also in the crop of the fowl. The small bowels of the ox and of the pig are sometimes the seat of tubercular lesions, the large rarely; but intestinal lesions are very common in poultry and in birds, though their character differs materially from that of similar lesions in the bowels of other animals. In these creatures an intra-mural growth is common, and the neoplasms frequently attain an enormous size, one which I here exhibit weighing when it was fresh about 2½ ounces. These nodules, on section, frequently present a stratified arrangement, and while in some instances they produce irregular excrescences on the peritoneal surface, they, on the other hand, sometimes extrude on the surface of the mucosa, and occasionally soften and produce tubercular ulcers. They frequently occlude the lumen of the tube, and in the worst cases the mesentery is thickly bestudded with nodular growths. A remarkable lesion, never seen in mammals, is sometimes met with in the intestines of poultry, viz., the throwing out of layers of a croupous exudate of a yellow colour, sometimes intermingled with blood coagula, which occasionally form a cylindrical cast of the intestines and completely fill the cavity thereof.

In the respiratory tract we find that the nasal mucous membrane and the chambers are sometimes the seat of tuberculosis in the ox. While in poultry the nostril is one of the most common sites—and the yellow, croupous-like exudate, so characteristic of the affection, is, as also in the case of the mouth, pharynx, conjunctiva, and even the external aural canal, frequently confounded with and spoken of as *diphtheria* and *croup*.

In the larynx not only do we see the development of large tuberculous tumours which are occasionally pedunculated, but we frequently see distinct ulcerations and vegetations, though one medical pathologist has gone so far as to claim that the absence of laryngeal, tracheal, and bronchial ulceration in animals is one of the characteristic differences between animal and human tuberculosis. Undoubtedly bronchial and tracheal lesions, especially ulceration, are in most cases secondary to pulmonary. They are, however, occasionally seen independently of such lesions.

In the lungs the tubercular processes resemble in their distribution and in their characters those with which you are all familiar as occurring in the lungs of the human subject; it is sufficient for me here to say that there is not one single lesion discoverable in the lungs of man that has not its parallel in these organs in animals; and although some writers have endeavoured to draw fine distinctions in this respect, their distinctions are only such as are due to anatomical and histological peculiarities.

I observe that Dr Creighton speaks particularly of tubercular bronchiectasis. I have no hesitation in saying that all such conditions are secondary to pulmonary cavities, or rather are a continuation or a result thereof. In poultry, a remarkable pulmonary condition is sometimes seen, viz., the substitution of the entire lung-tissue by a stratified plastic exudate, similar to that sometimes seen in the intestines, and which, in some instances, forms an accurate mould of the lung, the bronchial tubes and vessels being left intact.

The structure of the heart is marvellously exempted from tubercular lesions, and while I have occasionally seen evidence of circumscribed interstitial lesions, I have only once seen the cardiac muscle the seat of tubercle, and even in that case the lesions were not very characteristic. I exhibit, however, a remarkable specimen, which shows that invasion of the auricle has been imminent.

Tubercular myositis, as affecting the voluntary muscles and as an independent lesion, is of extreme rarity; at least, if such lesions exist, they are of microscopical dimensions only. I have, however, met with myositis in the voluntary muscles as an accompaniment of, and resulting from arthritis and periostitis; and tubercular lesions in the tracheal and laryngeal muscles are commonly seen.

Tubercular ostitis is of comparative frequency, as is also arthritis, but the latter is, in the majority of instances, secondary to the former,—extension being easy, in view of the fact that the process usually commences in the extremities of the long bones (see femur of pig exhibited). The process in some cases goes on very rapidly, the softened products finding their way into the joints through cloacæ situated immediately below the edges of the articular layers of the bone, and sometimes penetrating the head of the bone. The hocks, knees, and stifles are most largely the seat of tuberculous lesions, and, as shown in a specimen of the hock which I exhibit, the identity of the joint becomes practically lost, and in the end there remains a mere shell pierced in every direction by cloacæ, and through which the softened products find their way to the surface by means of sinuses. In the progress of the lesion, the integument and contiguous structures, the periosteum, ligaments, connective tissue and muscles, all become involved, as does also the new interstitial tissue formed as the result of the inflammatory processes.

In the flat and irregular bones, *i.e.*, the vertebræ and their processes and the cranial bones, the diplœ is first affected, the compact structure ultimately yielding to the eccentric pressure exerted by the new growths and finally becoming pierced by cloacæ.

Cutaneous and subcutaneous lesions are rare in mammals, except in association with the softening processes which go on in underlying organs, such as lymphatic glands, bones and joints. In connexion with glands, tubercular ulcers are sometimes formed on the skin. I have on several occasions seen what appeared to be tubercular lesions in the subcutaneous tissue and ligamentous structures of the lower part of the leg of the ox; but as these have been generally brought to light in the process of preparing the legs for sale, it has been difficult to say, in the parboiled condition of the structures, whether the lesions were those of tuberculosis. Such a leg, obtained by the inspector of the abattoir in the Tripery last week, I introduce to your notice.

In the feathered tribe several remarkable phenomena are met with in the structures under consideration, as also in the comb, the wattles, and the feet. In the intermuscular tissue, flattened oval bodies, about the size of a lentil seed and of a yellow colour, are in some birds seen in tolerable abundance; they constitute the so-called tuberculo-diphtheria of some French authors. In the subcutaneous tissue covering the sternum extensive lesions, which sometimes involve the skin, are occasionally seen. Their localization in this region seems to be determined by the irritation of the perch.

The comb and wattles of the cock, and now and then of the hen, attain an enormous size, become hard and unyielding, and on section masses of the yellow exudate already alluded to are exposed. A similar condition occurs in the foot, producing deformity of that organ, and occasionally being associated with the formation of superficial ulcers on the plantar surface, through which the softened products are discharged and probably open up another source of contamination. A foot, removed from the body of a hen, I introduce to your notice.

The course of scrofula in mammals is a slow one, and the same is to some extent true of tuberculosis. In the case of the latter, however, we find that it sometimes runs a chronic, and at others a very acute course, while in either case it may become infective. Thus, when involving serous membranes only, and as a primary process, it usually runs a slow process. It may do the same in the case of organs, but in these the tendency is towards a more or less rapid course; but so long as extension takes place only by means of carrier-cells, or by the medium of the lymphatics, its course is comparatively slow in all structures. Once, however, the tubercular products gain access to the blood, all the organs of the body become quickly invaded.

The systemic effects exerted by tuberculosis depend upon the extent of the lesions, the degree of infectivity, the nature of the organs involved, the strength of the affected animal, and the tendency, or the contrary, to rapid softening. If the serous membranes only are involved, with a limited number of the lymphatic glands, and calcareous changes predominate, the host may appear to be in a perfect state of health,—may, in fact, present to the eye a picture of blooming health,—while, on post-mortem examination, many pounds' weight of tubercular new formations may be found in the abdomen or the thorax, or both; yet the carcase may be laden with fat, and the muscular tissue be perfect in its physical aspects.

On other occasions, and more particularly in young animals, the withdrawal of so much nutrition for the purposes of the neoplasms, tells its tale upon the system; and as in the case of all other parasitical diseases, the body becomes emaciated, the skin bound to the subjacent tissues, the coat dry and erect, the eye retracted in the orbit, the mucous membrane anæmic, and the blood impoverished. The animal becomes weak, debilitated, and dropsical, and ultimately dies from pure inanition, its carcase remarkable only for its extreme leanness, and the veins partly filled with imperfect clots of a pale colour. I have often thought, on looking at a cow or calf in this condition, that if one could clothe the body in a human skin the tubercular cachexia would be as marked as it is in the case of the human subject.

In practice, we frequently find that in pregnant cows a halt in the onward march of the disease is called, but its progress again proceeds with redoubled speed after the act of parturition; and so, too, after æstral excitement has subsided.

When acute processes are in the ascendency, the febrile state becomes marked, a temperature of 105°–107°, or even 108° F., being frequently reached and maintained for days or even weeks; and as a result of this, rapid emaciation sets in and there is marked interference with the normal functions, with the exception (in many instances) of one, and that unfortunately the most important, viz., lactation.

Lastly, we not infrequently find that these acute symptoms subside, and the animal is apparently restored to perfect health and may become extremely fat. Subsequent investigation shows that the tubercular processes have been arrested or become obsolete. The clinical phenomena depend upon the organs involved. Those of pulmonary and intestinal phthisis, tabes mesenterica, tubercular meningitis, peritonitis, metritis, nephritis, öophoritis, and orchitis, present parallel characters to those seen in similar conditions in the human subject.

In their macroscopical characters the tubercular lesions of the lower animals are in the main the counterpart of those with which you are familiar in man—as Mr Creighton puts it, the mimicry is perfect. On the serosæ we see, in the initial stages, the vascular pile characteristic of other infective processes; a little later we find that the villous growths have become converted into miliary and submiliary nodules of a pale colour and of a translucent aspect; anon, the colour changes to a gray or a yellow, and the nodule becomes firmer and more defined in extent. Individual nodules may be suspended from the surface of the membrane by delicate fibrous bands or united to neighbouring nodules by a new interstitial growth, or a number of nodules may become united together by new material and form enormous aggregations, which may press upon and interfere with the functions of contiguous organs, or may displace them from their normal position—*e.g.*, the heart and lungs—and even cause partial atrophy thereof, and may become the media of parietal and visceral adhesions. Polypoid, sessile, and festooned growths of every variety and extent are to be met with in different animals, and notably in the ox. (See specimens on table.)

In the parenchyma and connective tissue of organs, and in the stroma and lining of glands, distinct miliary nodules may develop and may ultimately in these situations, by the addition of inflammatory products, form immense neoplasms, which in the end may bring about positive substitution of the normal tissues by adventitious matter, and thus annihilate the organ both functionally and structurally.

Thus, portions of a lung may be entirely converted into tubercular neoplasms and adventitious tissue (see specimen),—nay, more, the whole of one of the large lobes may be structurally wiped out and the parenchyma be replaced by masses of calcareous matter, which, as in the case of the dried section of lung I exhibit, may become intermixed with true adipose matter,—the only condition, I may observe, under which I have ever seen fat, as fat, deposited in the lungs.

Tubercular cavities, tubercular cysts (if I may be allowed the expression), tubercular ulcers and cicatrices, are, each in their turn, met with in the course of our necroscopies; and, as shown by the specimen contained in the small bottles on the table, the organs, such as the liver, become so invaded and so altered in structure as to render it an easy matter to remove the parenchyma and isolate the tubercular nodules by levigation.

And now, Mr President and Gentlemen, allow me, as a corollary to these necessarily discursive remarks, to direct your attention for a brief space to the economical and sanitary aspects of this question. Economically, I have no hesitation in saying that there is no disease at present afflicting the lower animals that causes such serious losses to the community, or that diminishes the food supply so largely in these islands, as does tuberculosis, and yet our legislative bodies stand idly by and take no steps whatever to arrest its progress. I do not believe that I am exaggerating when I say that tuberculosis kills more cattle and birds than do all the sporadic diseases to which they are subject put together. Poultry-yards are decimated, herds of cattle and swine are practically annihilated by this fell destroyer, and not infrequently the stock-owner sees his rent lying in the farmyard in the shape of the carcasses of pigs and poultry, and his pastures converted into grave-yards for his cattle.

I am within the mark when I say that, on an average, there is not one day of the whole 365 comprised in our year in which I could not find a tuberculous subject in our abattoir, and what is of graver significance, scarcely a regular market day passes in which I do not see several consumptive cattle; nor can I pay my round of periodical visits to our byres without detecting at a glance a smaller or larger number of such animals. When I tell you that, on one occasion lately, we found no less than three cows from one byre standing in one batch in our cattle market, and that on another occasion I was asked to look at two young cows far gone in consumption (one of them, in fact, dying) that had only been purchased fourteen days and a month respectively, you may be able to realize the extent to which the malady exists. It is the fashion to speak of this disease as having its genesis in our byres; in 19 cases out of 20 it is in existence when the animals are brought in, and though we may grant that the conditions under which they are placed have much to do with the rapid progress of the malady, we cannot accept the optimistic view recently taken by the *Lancet*, viz., "That under improved sanitary arrangements the number of cases of tuberculosis in our town dairies will become materially diminished." Such favourable influences may retard the development of the disease and modify its results, but it cannot influence its origin. Not only do we find cattle and their carcasses affected by this disease exposed for sale, but, as Dr Littlejohn will also tell you, we find flesh prepared by the mincer, for the purpose of making sausage, in sausage manufactories.

It is for such bodies as the one I now have the privilege of addressing to take up this question, and pronounce an opinion upon its gravity without fear or favour; you equally with us are the natural guardians of the health of the community, What are you doing now? what have you done in the past to bring this matter before the legislative bodies of the country? Much credit is sometimes taken by medical writers for the share taken by members of your profession in bringing these matters to the light of day, and only in its last issue the editor of the *Lancet* in an annotation says, "That even at the present juncture, when such questions as the development of scarlatina (mark the word development) from a bovine disease, or the relation of bovine to human tuberculosis, have arisen, it has been the members of the medical profession who have taken the initiative in studying and discussing the subject." So far as the first part of this remarkable paragraph is concerned I acknowledge its accuracy, for I know of no reponsible member of my profession who would waste his time and energies in trying to discover that which never existed. But in reference to the closing remarks of this statement I distinctly join issue with its author. In the essay already referred to, as having been read by me in Glasgow in the year 1872, I make the statement, "that the subject is one of immense importance to the public, because it has a very close connexion with the great social problem of providing our masses with a sufficiency of nourishing and healthy food." "To the stock-breeder and agriculturist it is even of greater import, as by the spread of the disease—and it is undoubtedly on the increase—his hopes are rendered fallacious, and his profits materially curtailed;" and again, "if the contagious character of tuberculosis is established, another (though perhaps a remote one) source of contamination is opened up before us, viz., the ingestion by persons or animals of raw or improperly cooked meat, or more probably, livers or other internal organs from animals that have suffered from this affection."

In the *Four Bovine Scourges*, published in 1879, I return to the charge, and I there say, "under any circumstances the internal organs should be destroyed, and it is a matter of grave consideration whether the flesh of an animal suffering from even a slight degree of tuberculosis should not be condemned." "As to the use of milk from animals in which tubercle is suspected to exist, no two opinions can be held." "I have known gentlemen sell cows for dairy purposes in whose systems they have been told tubercle existed; and cow-keepers do not always hesitate to add the products of a tuberculous cow to the bulk of their daily yield of milk." "It would be far better to give compensation and have even a suspected animal destroyed, than to allow her to remain in a herd or byre with the probability of spreading the disease to her neighbours and of poisoning the consumers of milk."

In 1883 the subject of tuberculosis was discussed at a meeting of the National Veterinary Association held in London, the discussion being initiated by the reading of a paper by Mr J. H. Cox of the Army Veterinary Department, and after various speakers had expressed their opinions on the nature of the malady and its economical and social importance, the following resolution was unanimously carried, viz., "That in the opinion of this meeting the Privy Council should include tuberculosis in the Contagious Diseases (Animals) Act." In September 1883 the subject was brought before the International Veterinary Congress, held at Brussels, by the reading of a paper prepared by Van Hertsen of Brussels, Fleming of London, and Lydtin of Baden.

In 1884 the subject was again discussed at the second annual meeting of the National Veterinary Association held in Manchester, Professor M'Call of Glasgow this time being the introducer, and I had the pleasure of proposing a resolution to the effect, "That the flesh of animals which have suffered from tuberculosis (as well as from certain other diseases enumerated), or any disease likely to prove dangerous to the human subject, should be condemned." In 1885 Mr J. Sutcliffe Hurndale, F.R.C.V.S., read a paper on the subject before the Liverpool Medico-Chirurgical Society.

In 1885 Professor Bang of Copenhagen read a paper on "Tuberculosis of the Udder and its effects on the Milk of Cows" before the International Medical Congress at Copenhagen. Professor M'Fadyean, with Dr Woodhead, made some important remarks on the subject at the meeting of the National Veterinary Medical Association held in Edinburgh in 1886, and again at Dublin last year, while in July next the disease is to be made the subject of discussion at another meeting of the International Veterinary Congress, to be held in Paris. Of Continental veterinary investigators, it may be said that their name is legion; and Virchow himself declared that Gerlach was the first to draw attention to the infectivity of milk and to the danger attending on its consumption by human beings.

In view of these facts, I think, Sir, the editor of the *Lancet* has been a little hasty in his remarks, and I think, further, that the members of the medical profession cannot show a more imposing array of work, in this direction at least, during the period mentioned, than that which has been performed by those of the veterinary profession.

Dr Creighton has, so far as I know, done more than other members of the medical profession in the matter of endeavouring to prove the identity of human and animal tuberculosis; but I am inclined to think that he has endeavoured to prove too much when he draws positive conclusions from the anatomical identity of the lesions in a series of cases occurring in man; but you are the best judges of this, and in order to enable you to form a judgment on this basis, I have collected, and brought here for your examination, a large number of sections of different organs from the ox, the pig, and the fowl. While Dr C. relies on the morphological identity, page 5, and says, "So remarkable is *the structural mimicry that resides in infection*, that the disease communicated to man reproduces the special anatomical characters of the bovine disease with a surprising degree of accuracy," he, nevertheless, draws a distinction between the bovine and human tuberculosis in the direction of the rapidity or otherwise with which the disease runs its course, and in reference to its heredity. "The specific tuberculous disease of the cow or ox," he says, "is a very slow disease, which may escape notice for years, and not seriously interfere with nutrition, and it is probably inherited; the corresponding disease in man is of the nature of a more or less acute infective disease, with the clinical symptoms of an infection predominating." How far he is justified in arriving at such conclusions may be judged by what I have already said to you on these points.

Our only means of establishing an absolute identity between the disease in man and animals are very unsatisfactory. We have only presumptive evidence to guide us, *i.e.*, etiological, morphological, pathological, and clinical analogy, but we cannot pass this form of evidence by. If we consider the etiological entity, we shall find that some biologists argue against the identity on the ground of morphological and other differences in the bacillus. Dr Klein particularly does so; but in reference to his views Dr Woodhead and Professor M'Fadyean justly point out that, even as between the bacilli in human sputum and that obtained from a nodule in the lung, there appears to be a difference in point of size, and that even cultivation in solid and liquid media exercises a modifying influence in this direction. These gentlemen also direct attention to the differences which are observed in the bacilli of anthrax when obtained from different species of animals. I apprehend that any distinction drawn from such a source would be a very fine distinction, seeing that the characteristics not only of animal but of vegetable life are materially modified by its environments.

Koch—*Syd. Soc.'s Microparasites in Disease*, page 197—says, "they must be regarded as identical diseases, on account of the identity of the parasites to which they are due."

Another ground which has been taken up with the object of disproving identity has been the clinical differences of the disease as observed in man and animals. If this alone were sufficient to disprove identity, then might we at once say that glanders, rabies, variola, foot-and-mouth disease, and anthrax had nothing in common in man and animals; and further, that the foot-and-mouth disease of cattle was not identical with that of the sheep or the pig, or that there were several forms of this disease or of swine fever because of the variations in the clinical manifestations and in the resulting lesions.

Macroscopical dissimilarities have also been quoted in proof of the absence of identity, and some remarkable statements have been made under this head. As well might the differences seen in the lesions of glanders in the dog and the horse be quoted to disprove identity; and even if the lesions of tuberculosis frequently present a different aspect in man as compared with the lesions in cattle, this difference may be accounted for by taking into consideration the differences in the histological characters of the tissues and in the composition of the food. Diphtheria is usually seen in man in the throat. I have seen splendid examples of the affection in the larynx and in the trachea of cows.

I have said that much of the evidence upon which we rely to prove the identity of the disease in man and animals is of a presumptive character; but how often does it happen that this form of evidence is relied on to prove the identity of other diseases, and even to prove the ordinary occurrences of human life?

In Mr Cox's paper, read at the National Veterinary Association, three cases of probable transmission of the disease from animals to man are quoted. In the first, several members of the family of a dealer in "wasters" were supposed to have contracted the disease by drinking the milk of these purchases; in the second, a cousin of the author's, into whose dietary milk entered largely, became consumptive, and during the progress of the disease in her system it was discovered that two out of the three cows which formed the stock on the farm were suffering from tuberculosis; and in the third case, the son of a farmer was supposed to have become inoculated through the medium of sores, which existed on his hands, by the discharges from the sores on the teats of a tuberculous cow.

In the course of the discussion on Mr Cox's paper, Mr Hopkins, F.R.C.V.S., Manchester, referred to an instance where two female members of a family who drank large quantities of milk became the victims of consumption, while their brothers, who preferred whisky to milk, remained healthy. It was discovered that tuberculosis existed to a large extent in the cows from which the sisters had obtained their milk supply.

In the paper by Van Hertsen, Fleming, and Lydtin, already referred to, there is a remarkable chart, showing the statistical relationship which existed in the administrative districts in the Grand Duchy of Baden in 1881 between tuberculosis in man and animals.

In 1871, when I first arrived in Edinburgh, milk was obtained for my family from a vendor who hailed from the country. One morning, three of my children (up to that time as healthy as children could well be) were suddenly attacked with violent vomiting and purging shortly after they had partaken of breakfast. Two of the children quickly recovered, but the youngest never rallied, and in a few weeks the late Dr Stevenson Smith declared that he was suffering from tabes mesenterica, and, I believe, Dr Taylor, who also saw the child, concurred in this view. All the children had partaken largely of milk, but, unfortunately, the youngest had received the biggest share; and further, after having vomited, he was supplied by the nurse with another drink. Both Dr Stevenson Smith and myself tasted the milk and agreed that there was something very unusual in its quality.

Dr Fleming, in a paper read by him some years ago at a meeting of the Eastern Counties Veterinary Medical Association, directed attention to the case of the youngest child of a surgeon in the United States, who became the subject of tubercular meningitis contracted from a cow that had been declared by the veterinary surgeon in attendance to be the subject of tuberculosis.

Milk is undoubtedly the medium through which transmission is most likely to take place; and judging from the numerous experiments which have been made, both by ingestion and inoculation, there can be little doubt as to the frequency with which it possesses viruliferous properties. But, while admitting the extreme danger that may arise from consuming the milk of tuberculous animals, I am, nevertheless, of opinion that the degree of danger is often exaggerated.

In 1884, in a letter written to Mr Hurndall, I stated, in reply to a query from that gentleman, that I was strongly of opinion that tuberculosis could not be transmitted through the medium of milk from the cow to man unless the affected animal was the subject of udder lesions. Dr Imlach of Liverpool had shortly before the date of this letter endeavoured to prove by experiment that the danger of such transmission was chimerical; but from a perusal of the particulars of his experiments, I had no hesitation in saying to Mr Hurndall that they were of no value whatever in determining the question at issue, as it was quite evident that no care had been taken to select a cow with well-marked udder lesions.

Professor Bang, in the paper read by him at the Medical Congress at Copenhagen in 1885, made some very important statements in reference to the infectivity of milk, and of cream separated therefrom by the centrifugal system; but, in the course of his remarks he gave utterance to two statements of a positive character which I could not endorse. These statements were to the effect, firstly, that he, Professor Bang, was able, as a rule, to diagnose tubercular mastitis; secondly, that even from the healthy parts of the udder of tuberculous cows suffering from mammitis the milk was infective.

In reference to the first statement, I remarked to my class at the time that I could not undertake to diagnose with accuracy tubercular mammitis in every case, nor even in a majority of cases.

In reference to the second statement, I made the same remarks as those which I addressed on the subject to Mr Hurndall, and I am in a position to point out to you how such conclusions might well be falsified. In several specimens on the table, removed from the udder of a tuberculous cow, you have good examples of an eruptive tuberculosis on the mucous membrane of the galactopherous sinuses, without the slightest evidence of the usual concomitants of tubercular mammitis in the shape of induration of the udder substance; indeed, I may say that no veterinary surgeon could, during life, have diagnosed the existence of tubercular mastitis without the aid of the microscope.

Since I obtained these specimens I have received from Professor Bang a most important letter, which I shall read to you.[1]

The results obtained by Professor Bang in reference to the action of heat on the bacilli are similar to those obtained by other authorities, and show how little dependence there is to be placed on the supposed disinfective properties of such a degree of heat as most people are likely to apply to milk they are about to use for dietetic purposes.

I know not, Mr President, what tone the discussion on this subject is likely to assume in this meeting to-night, but I would suggest that particular attention should be given to the question, in the first place, of the probability of bovine tuberculosis being transmitted to man by the ingestion of the flesh, the milk, or the viscera of tuberculous animals; in the second place, to the advisability or otherwise of providing for a more thorough and scientific system of inspection at our slaughter-houses; in the third place, to the advisability of memorializing the Privy Council to include tuberculosis amongst the contagious diseases of animals, and thus giving veterinary inspectors the power of seizing all tuberculous cows, or even suspected animals, they may find in byres, markets, or fairs; in the fourth place, to the advisability or otherwise of condemning the flesh of animals affected with tuberculosis, even though it may be of a limited extent and localized in the serosæ.

Personally my views in reference to these questions are now, as they have been for years, pronouncedly in the affirmative, and it is only right that I should point out to you that as the law now stands we have no power to deal with this matter, and even on the question of the milk supply, the old saying "that a coach and four can be driven through any Act of Parliament or any law" was amply verified in a recent prosecution at Paisley, where the defendant escaped punishment, as so often happens in these cases, through the medium of a technical informality in the charge.

In reference to my individual power of dealing with cows that are manifestly the subject of tuberculosis, I occupy probably a unique position amongst my brother inspectors, inasmuch as by the wisdom of the Magistrates of this city I am empowered to do what seems to me best in such cases.

[1] Professor Bang says that, as the outcome of a series of experiments recently carried out by him, he has now come to the conclusion that the milk of tuberculous cows and tuberculous women, in which there are no lesions in the mammary gland, only exceptionally contains the contagium. Professor Bang at the same time, however, points out that the milk from tuberculous udders is extremely dangerous, and that the tubercle bacilli are to be found, not only in the milk itself but in the cream, butter-milk, and butter made from it; and that such milk is sometimes infective by ingestion, even after exposure to 65° C. of heat, and by injection into the peritoneal cavity after exposure to 80° C.

Professor M'Fadyean (called upon by the President) said,—It is with considerable diffidence that I rise to make the first contribution to this discussion, because I feel that I am standing in the way of many who probably have much to say that would be more to the point and more interesting. I may say that I think this is, to comparative pathologists, the disease which occupies the first place in point of interest. It is, I think, in some measure a reproach to the medical profession, that it has not up to this time attracted very great attention; and it seems to me almost a marvel that the allegation recently made that scarlatina in a human subject might be due to a bovine disease identical with scarlatina should have instantly attracted the attention of the medical profession, and even of the public, while this subject, which is of infinitely greater importance, is hardly ever mentioned. It may be asked in the first place—What are the proofs of the identity of tuberculosis of man and tuberculosis of the lower animals? Well, the evidence on that head is, I think, overwhelming. I think it may be said to divide itself naturally into—*first*, the experimental evidence; *secondly*, the histological evidence; and, *thirdly*, the evidence relating to the identity of the bacilli which modern methods enable us to discover in all tubercular growths. With reference to the first of these—the experimental evidence—I think that it is absolutely conclusive. There is but one step wanting, and that is simply that nobody has deliberately and of set purpose communicated tuberculosis to human beings by giving them tubercular flesh, but the converse experiment has frequently been made. An experiment which invariably succeeds in a large proportion of cases is that of attempting to communicate tuberculosis to the lower animals by feeding them with tubercular meat. With reference to the histological evidence, comparison of the minute structure of bovine tubercle with that found in the human subject certainly of itself would to me almost establish the identity of the two lesions. In particular organs of bovine animals there are types of tubercular structure that one does not often meet within human organs. That is true principally of the chronic tubercular growth, which is found so commonly in connexion with the serous membranes of the bovine animals; but in these animals when an acute tuberculosis is set up, then the lesion—as, for example, the structure of the tubercle which is formed in the lung—is absolutely identical with the tubercle which is formed in the human lung. The same applies also to the tuberculosis of horses, of which, as Principal Walley mentioned, I have lately had an opportunity of examining two cases. I am not sure that an examination of the histology of the so-called tuberculosis of fowls would afford very strong evidence as to the identity of that with human or bovine tuberculosis. All the specimens that I myself possess of avian tuberculosis exhibit very considerable difference,—for example, I have not in any of the specimens I have yet examined found giant cells in the lesions. With regard to the identity of the bacillus, I believe that the evidence accumulated on that head is sufficient to show that there is no recognisable difference in the characters of the tubercle bacillus when obtained from growths from bovine animals and when obtained from a human being. There is hardly any force at all in the objection which Klein urges regarding the difference of size of the tubercle bacillus as found in man and in animals. Klein himself points out that the bacillus of anthrax exhibits a very considerable difference in size when taken from different animals, and only a few pages further on he cites this difference of size of the tubercle bacillus as being evidence of the non-identity of the tubercle bacillus of men and animals. I think, then, that it must be held that it has already been abundantly proved that tuberculosis of animals and that of the human subject are identical, and it therefore is of the utmost importance to consider what are the dangers and what likelihood there is that tuberculosis of the lower animals may be communicated to human beings. That danger appears to exist, in the first place, in connexion with the consumption of tubercular food, and, in the second place, in the consumption of milk from tubercular cows, and I shall only address myself, and that in a very few sentences, to the last of these. My own attention was first strongly turned towards this after reading Professor Bang's paper before the International Medical Congress. Since that I have been collecting all the udders I could get; and I am certain that if I had the opportunity of submitting certain specimens which I possess of tuberculosis of the mammary gland under the microscope, I should soon be able to convince any one—perhaps sooner than by a long course of reasoning—of the supreme importance of this subject. About two years ago I procured a specimen of tuberculosis of the udder of the cow. This animal, I may say in passing, was in prime condition, and it had only got a small patch of tuberculosis in the lung in addition to the tuberculosis of the udder; yet almost any section taken from that cow's udder reveals the tubercle bacilli in inconceivable numbers. That cow was giving milk up till the day of slaughter, and it was not killed for disease, but because it was in a condition regarded as prime fat. In concluding, I would like to say that I think medical practitioners would not only find it very interesting, but human and veterinary pathology would benefit greatly, if they paid greater attention to the diseases of the lower animals. As illustrating that, I may be allowed to refer to a very interesting paper, detailing the results of certain experiments by Dr Philip, which I read a few days ago, these experiments being designed to show that the cachexia which is present in tuberculosis of the human subject is due probably to some irritant material which is secreted or formed by the tubercular bacillus. It may be a hint, if I say that one has no difficulty in finding in the Edinburgh abbatoir cows from which one can remove perhaps fourteen pounds or more of actual tubercular formation, every microscopic section of which teems with bacilli, and yet such animals may not have exhibited any cachexia at all. That may be a hint to Dr Philip as to where he can get a large quantity of this supposed irritant material with which to experiment.

Prof. Greenfield, on being called upon by the President—Mr President, I would very much prefer to postpone any remarks I may have to make until those gentlemen who have been specially asked to take part in the discussion—amongst others Dr Woodhead —have expressed their views.

Dr Woodhead—Mr President and Gentlemen, I also should like to postpone my remarks, but Dr Cathcart asked me to take part in this discussion. Having placed myself in his hands, I must speak when I am called upon. I listened with very great interest to Principal Walley's paper, and I must say that I gathered a number of very interesting facts from it. At the same time, I felt perhaps that he was scarcely doing the doctors justice—that he was detracting just a little too much from our side of the question. However, I know that there is nothing further from Principal Walley's thoughts, and therefore I will leave that subject. In the first place, I should like to say a few words on what was introduced by Principal Walley as to the identity of the condition in man and in animals. I know that Klein insisted, as you have already heard, on certain differences between the bacilli as found in man and as found in cattle. Now, in order that I might try to convince myself, I took a series of specimens from human and bovine organs, in which the bacilli were treated in exactly the same way. I examined these specimens carefully, and I then asked several of my friends to examine them, and to tell me, if they possibly could, which was from man and which was from the cow. In almost every case it was found to be absolutely impossible to distinguish between them. Klein says that the bacilli are a third less in bovine tuberculosis than they are in man. Now, I found that it was impossible to distinguish them—even using very high powers—to distinguish the one from the other. There might be slight differences, but equal differences occurred in the same specimen. Then, too, when I was doing this, I also examined for the position of those bacilli, because Klein holds that there is a very distinct difference in the position of the bacilli in bovine and in human tuberculosis. He holds that the majority of bacilli are in the cells in bovine tuberculosis, and that they are between the cells in human tuberculosis. I find that other authorities have taken this up—Dr Sutton among others, and, of course, Koch, from whom a great part of our information on this subject is derived—and these authorities distinctly say that in both human and bovine tuberculosis you may find the bacilli both in the cells and in the spaces; and as a matter of fact, that is so. Take any specimens you please, you will find bacilli in both positions in both cases. Even, however, though this were not so, one could readily understand that there might be differences in the rapidity with which the tubercular growths advance. One could readily understand that if you have rapidly growing bacilli —as we find in a great number of conditions, if the advance is rapid—the tendency to attack the cells would be much greater; and, as a matter of fact, we always find wherever the advance is taking place rapidly, that the attack of the bacilli upon the cells invariably goes on. Principal Walley has already referred to the growth in different media, which I consider to be a very important point in considering this subject, and he has supplied us with a great many specimens, and a good deal of evidence, as to the identity of the two conditions. Klein, in a series of experiments reported to the Local Government Board in 1886, gives the action of bacilli taken from a human subject and inoculated into fowls; also taken from a cow and inoculated into fowls and into guinea-pigs; and also from fowls to guinea-pig; and he finds the following state of affairs,— that you can inoculate fowls with tubercle taken from a human subject. In the same way you can inoculate from a guinea-pig to a cow, but when you begin to cross these—that is, when you begin to inoculate from a cow to fowls—then your experiments break down. Now, in connexion with this, I should like to draw attention to one fact. Koch in his classic experiments brought out this point very strongly—that you can cultivate tubercle bacilli at a temperature of 86° to 105°, but that beyond these limits it is exceedingly difficult to get a growth of tubercle bacilli. Now, this appears a somewhat important fact, when you come to consider the temperature of the animals on which you are experimenting. For instance, one could readily understand that you can modify the virulence, the activity of these bacilli and their power of growth. If you can modify these outside the body by temperature, you can also to a certain extent modify them by the temperature in the animals. I asked Professor M'Fadyean to-day to give me the normal temperature of some of these animals. I took a note at the time. In the cow the temperature is 101° to 102°. In the hen—and this is subject to correction—it is from 104° to 105°. In a calf about 103°; pig, same as cattle; and horse, 101°. Now, it appears that if you could only inoculate fowls with human tuberculosis—and you cannot transmit from the cow to the fowl or (probably) from the fowl to the cow—it would appear that temperature might have a very important effect on the vitality, not perhaps immediately, but on the virulent power of these bacilli. By increasing the temperature of the subject, you might diminish the power of growth and of attacking the tissues possessed by these bacilli. Of course this is only in connexion with the experiments carried out, and as a partial explanation at least of what may actually take place. Then as to the transmission of tuberculosis from the bovine subject to the human subject. Principal Walley has already told us that calves are subject to tubercular ulceration of the first parts of the alimentary duct. Now, when Professor M'Fadyean and I were working at this subject, we happened to come across a series of cases in a very localized position. We had to do with a dairy in which we found evidence of tuberculosis in the udders of three cows. These three cows were giving milk, and this milk was going into a large establishment, and it was going into this establishment only. We found that in this establishment the death-rate from phthisis was in one year 40 per cent., and in another 30 per cent. of the whole deaths. At the same time there was a series of experiments going on which were useful to us, although not so useful to the people who were carrying them on. The pigs were being fed with the milk from these same cattle, and these pigs were—a large number of them—suffering from what was evidently a tubercular condition of the lungs; and we learned that a number of these pigs were killed, and the most beautiful tubercle was found in the lungs, in the pleura, and in one or two instances in the small and large intestines. Now these pigs were being fed on the milk from this cow-house, whereas other pigs, which otherwise were under the same conditions—the only exception being that they did not get the milk—escaped tuberculosis. These experiments were partly carried on before we went, and were continued after we left, but the pigs had the milk supply cut off simply as the result of our visit; and as a result of this, the pigs brought up in the same place afterwards escaped tuberculosis. In Dublin a very similar fact was brought forward by Mr Headley, F.R.C.V.S. When we mentioned the first part of this experiment, Mr Headley informed us that a very large number of pigs had been treated in the same way within his own experience, and that the results were positive when the milk was taken, and negative when the milk was withheld, the same conditions being maintained otherwise; so that here we have a very important point in connexion with the milk from tubercular udders. It has been mentioned that tubercular cattle do not necessarily give tubercular milk, I think that Professor Bang has modified his position very greatly since the Copenhagen Congress, because at that time he held very strongly that it was not necessary that the udders should be tuberculous at all in order that bacilli should be found in the milk. However, he has since modified his position, and he could scarcely help this, taking into account the strong evidence given by Koch and others. In almost all these experiments where tubercular infection has been brought about through tubercular milk, it has been found that the udder has been affected, whereas in those cases where the results have been negative, irregular, or doubtful, sufficient attention has not been paid to this point; and in many cases the udders were undoubtedly not tubercular. Then there is one objection which is often raised—as to the identity of tuberculosis in the human subject and in cattle. It is held that in woman tubercular mammitis is extremely rare; in fact, I cannot hear of an authentic case,[1] whereas in cattle it is of common occurrence. I think, however, that here we should take into account the fact that lactation in cattle is carried on under very different conditions from what it is in the human subject; we know that lactation is prolonged very much longer in cattle—that is, in proportion to the time required—than it is in the human subject, and consequently the mammary gland is under conditions certainly not favourable to health, or at least very great strength. There is practically a very great over-development of the glandular function. In connexion with this I examined a series of glands from a tubercular udder, and I was astonished not only at the enormous amount of tuberculosis, but also at the very great rapidity with which it grows. There are one or two other points in Principal Walley's paper to which I should like to refer. One is with reference to the extension from the ovaries along the Fallopian tubes. It is rather curious, but true, that one month I had only six autopsies in the Sick Children's Hospital, and in three of those I found tubercular affection of the Fallopian tubes secondary to tuberculosis of the abdominal cavity. In only one of these was there any affection in the ovary itself; in the others the tubes were affected without any affection of the ovaries. I should also like to refer to the very great frequency with which tubercular enlargement of the mesenteric glands is met with in children, especially at an early stage of tuberculosis. In a very large number of cases, looking over the books, I find the enlargement of these mesenteric glands with very little affection in other parts of the body. There appears to be an absorption of the tubercular bacilli from the intestine. These are transmitted to the lymphatic glands, where they are apparently held as in a filter, beyond which they do not pass for some time; but one very frequently finds that you can follow a chain of glands—first, immediately behind the sternum; secondly, along the line of the sterno-mastoid, attended with very little tubercular affection in any other part. I have found bacilli in these glands where there has been practically little tuberculosis in any other part of the body. I have also examined a number of the glands from the roots of the lung where there has been tuberculosis in no other part of the body, and in these also it appears that the bacilli are taken up apparently by the lymphatics, passed on to the glands, where we find them in operation, and there set up morbid changes. Now we are sometimes told that in the lung there is a lymph circulation

[1] Since the above was given, Dr Cathcart has given me a reference to eight cases.

in one and sometimes in another direction, but as matter of fact one finds the course of the lymph circulation varies in different cases; in certain cases you have the glands affected primarily (where you have evidently an absorption from the lungs or from the intestine), or you may have these glands affected secondarily following the changes in the pericardium, as in the lung, or in tubercular ulceration of the intestine. Now, this is a somewhat important point, and I think it one on which sufficient stress is not laid. However, I am wandering from the subject of the danger of infection from contaminated animals. I really do feel, after what I have heard from Principal Walley and Professor M'Fadyean, that I cannot add very much of value to the debate, but I can say that, from what I have seen when I, along with Professor M'Fadyean, have been examining cattle for this tubercular condition of the udder, one feels that it is necessary that some such measures as those mentioned by Principal Walley should be resorted to. It seems to me that the health of the community could be very greatly improved by the adoption of some such measures as he has mentioned. One can give no idea of the very marked changes that are seen in the udders of cattle, and one would certainly not drink the milk from such cases, nor would one care to recommend it to one's patients if we always examined it microscopically. I can only add that I have listened with very great interest to the paper delivered to-night. I should like to have said more on the subject, but I feel that I should much prefer to hear the opinions of some other members of the Society.

Dr Littlejohn—Mr President, I think you and the Council deserve great credit for having asked a distinguished member of the sister profession to address us on this important subject. In fact, I think it would be of great importance if there was more complete union in such debates between the two professions of medicine and veterinary science, to which latter profession the success of one of our most distinguished physicians—I have heard him confess it himself—is largely due, he having at one time studied veterinary science, and even taken a licence in that profession. I confess I have been very much interested, indeed, with the statements made by the two professors and also by Dr Woodhead, and I think it cannot be doubted that this formidable disease is communicable from those lower animals to man. The question, of course, arises, To what extent? I was very much startled last week in presenting to our Health Committee an account of the animals which have been destroyed at the instance of our inspectors, and I have no doubt that Principal Walley's advice was taken with regard to many of these—to find that out of 42 animals which had been destroyed by our inspectors, 17 of these animals laboured under very advanced tuberculosis. I was exceedingly struck at that, and in looking over where these animals came from, I observed that only three came from the country—that the rest of these animals came from the town. I was exceedingly anxious to make out how many of these came from our town dairies. That I could not make out, because I was told that some were openly exposed in our public markets. I asked, however, about the worst case, and I found that it came from a dairy in the South Side of Edinburgh. I questioned our inspectors, who are not scientific men, but plain, practical men, guided mostly by the rule of thumb, and they said that the condition of this cow in this well-known dairy in the South Side was such that it was actually dying from consumption. Now, I think we must all agree with the Principal that something must be done in order to prevent the spread of this formidable disease. The question is, How is this to be done? Now, it can be done by putting our hands into our pockets. The Principal knows that we have succeeded in getting Government to pass a law by which compensation is given to the proprietors of animals affected with pleuro-pneumonia if they give timeous notice; for instance, if any dairyman observes a cow showing symptoms of affection from pleuro-pneumonia, he at once summons Principal Walley or the other inspectors, and they order the cow to be slaughtered; and if, on the cow being slaughtered, it is found to be suffering from pleuro-pneumonia, then three-fourths of the value of that animal is given to the man. Now this entails a very heavy rate on the part of the inhabitants, because I find that this last year the authorities of Edinburgh paid no less a sum than £800 in the way of compensation to enable us to stamp out this pleuro-pneumonia, and I have no doubt that Principal Walley in his reply will tell us how far we have been successful throughout the country in stamping out this disease. You can easily understand that any law about tuberculosis must not be a local law, because if you get the municipality to pass a local law, and if it don't extend to country districts, Edinburgh would become a perfect sink for the reception of all tuberculous cows, where owners would be sure of some compensation. Something must be done, and therefore we must approach the Privy Council to get them to put tuberculosis in the same category as this pleuro-pneumonia. Now I notice that the total mortality of Edinburgh last year was 4824. How many of these deaths occurred from phthisis? 508. And as I look back over the past twenty years I do not find that phthisis is increasing at all. Generally we have about 500 deaths from that cause in Edinburgh. Of course cases may escape us, because doctors may not always name the disease in order not to hurt the feelings of friends. I know that I have had people come to me after the death of a son from phthisis, and say, "What am I to do? because here the daughter of the family is going to be married, and if the cause of the death of this young man is put down as phthisis, it may injure her in the matrimonial market." This gives you a very good notion of the amount of consumption which we have in town. Some interesting remarks were made by Principal Walley as to rabbits being such an easy means of communicating this disease of tuberculosis. Could statistics not be framed as to the effects of eating rabbits upon the working classes of Manchester, Liverpool, and other large towns, where tons of these animals are consumed? Then Principal Walley says something as to the manner in which this disease is propagated. Now I hold that it is Professor Walley's duty, as our principal adviser, to insist upon the sanitary condition of the byres being greatly improved, and so greatly ameliorate the condition of the poor cows that are tied up for so many months in the byres. Another point deserves investigation, either by him or by others, and that is with regard to the sewage increasing the propagation of tuberculosis. We have the Craigentinny Meadows, the grass of which is partaken of so largely. Have we not the means of judging of the effect of sewage in spreading this disease? I may tell Professor Walley that the sooner he begins his researches the better, because every day increases the strength of the sewage which is passed down to Craigentinny. I can easily understand the increase, because as we introduce water-closets into our town, the refuse we get in our streets becomes of less value. When I began duty we got about 2s. 9d. per ton for the refuse which we took off our closes and streets. What do you think we get now? Twopence! And what is that owing to? That is owing to the rapid increase of sanitary conveniences in houses. So that tuberculosis, if influenced at all by sewage grass being given to cattle, ought to be greatly on the increase so far as Edinburgh is concerned. In looking over the records, I find that the mortality is not increasing. The great difficulty is, When are we to condemn the animal? It is all very well for us to discuss *ad infinitum* those difficult pathological questions, but the question comes to be, How are we to prevent the flesh of animals affected with tuberculosis being consumed? When are we to interfere? Principal Walley and I had to give evidence at Carlisle in regard to a magnificent animal which had been sold along with others, and this one happened to be killed; and when it was cut up, evidence of former tuberculosis was found in the shape of dried-up cretaceous exudation. The flesh was magnificent, and the whole appointments of the animal were perfectly good, and both Principal Walley and I stated at the trial that we could not see anything wrong with the flesh. In spite of that the whole herd was returned on the hands of the original owner in consequence of one of the best of the animals, when killed, showing traces of tuberculosis. Now, in winding up this discussion to-night, or at some future meeting, Principal Walley should tell us at what stage we are to interfere in destroying animals showing symptoms of this disease, and what is the machinery he would recommend the municipal authorities to adopt, because there is a great difference between pleuro-pneumonia and tuberculosis. The men at the gates of our slaughter-house can recognise an animal affected with pleuro-pneumonia, but I think I am right in saying that there may be a great difficulty in distinguishing an animal affected with tubercular disease. What I wish specially to bring before the notice of this Society is, if we get the Privy Council to place tuberculosis in the same category as pleuro-pneumonia, the ratepayers must look to it that they must put their hands in their pockets and pay heavily in order to stamp out this disease. I beg again to congratulate the Society on the very admirable paper we have had from Principal Walley.

Professor Greenfield—Mr President, I do not like to detain the Society at this late hour, as I came to listen only, and have really practically no original contribution to offer upon the subject. I listened with very great interest to Principal Walley's paper. I think it is of great interest and value, as bringing before the members of this Society so many facts and illustrations of the pathological anatomy of animal tuberculosis. At the same time, I should like to corroborate what Dr Woodhead has said with regard to the views which seemed to be expressed by Principal Walley as to the relations of the medical and veterinary professions in research upon this subject. I think he is mistaken in thinking that it is so recent a question amongst the medical profession, or that they have paid so little attention to it. It has certainly for the last ten years been one of the burning questions, and one of those to which the attention of scientific medical men has been very particularly directed. It would not be right to go into any detailed criticism of the pathological part of the investigations which Principal Walley has brought forward, or I should have liked to state some divergences in detail with regard to the incidence and characters of tuberculosis in certain animals. I think it would be best for me to refer only to one or two points which bear upon the main question, viz., the relations of animal and human tuberculosis. First of all, I may give a sort of expression of faith, founded on my own work on the subject during a number of years, and upon an endeavour to keep abreast of the very voluminous literature which has been devoted to it. As regards bovine and human tuberculosis, I cannot bring myself to believe, or rather I cannot find that there is any certain mark by which one can distinguish bovine from human tuberculosis. It is true that if one compares the two, one finds here and there that there are striking lesions which commonly occur in bovine tuberculosis, and which are rarely found in man. When one examines tuberculosis in other animals there are also occasionally striking apparent divergences; still, upon a comparison of the lesions found in man and numerous animals, I cannot discover that there is any point upon which one can fix, either in the anatomical characters or the character of the bacilli, by which one could with any certainty assert a difference in nature between animal and human tuberculosis. I think this is one of the questions upon which we still need information. Now, that being the case, we must go to other kinds of evidence if we want to determine a distinction between human and bovine tuberculosis; and it seems to me that the lines on which this investigation must proceed are—first of all, we might inoculate a number of animals from bovine animals and human beings respectively. These experiments might be of great value if we found a difference in the susceptibility of other animals to bovine and human tuberculosis respectively. Then there is another line of investigation in which, so far as I am aware, little has been done in inoculating cows from human subjects. Of course it will be said that the most satisfactory thing would be to inoculate the human subject from a calf or from a cow, but I am not aware that any extensive experiments of a converse nature have been attempted—that is to say, inoculation from human into bovine. If it were shown that such inoculation did produce lesions identical with those in spontaneous bovine tuberculosis, then we should have another great step towards establishing the identity of the disease. There is another point which Dr Littlejohn has, no doubt, had in view, and on which he will be able to give further information—that is, the question of the relative proportion of tuberculosis in population and tuberculosis in cows. Speaking of Dr Littlejohn's speech, certainly he has hit, as naturally from his large experience and acquaintance with these questions he would be likely to do, the difficult practical question which lies before sanitarians and veterinarians. How are you to tell when an animal has tuberculosis? and how are you to tell it early enough to prevent any mischief? That is a point on which there is extreme difficulty. Of course it is well known that the cows in dairies in towns are far more tuberculous than cows in the country. That is a well-known fact. Tuberculosis is extremely frequent in dairy cows in towns, but until a veterinary surgeon is prepared to assert that an animal has tuberculosis, I do not see what is to be done. I would ask Dr Littlejohn whether there are any powers for the inspection of dairies and condemnation of animals suffering from tuberculosis, and whether these powers might not do something towards meeting the difficulties. There is just one other point that I should like to mention. It would be of very great value if we knew the precise modes in which tuberculosis is transmitted from bovine animals to man, and knew exactly the course it runs. We might thus possibly distinguish two groups of cases: the one those which are transmitted from bovine animals, and the other those which are transmitted from man to man. I think it would be a point of very great importance in relation to this—if we could get more statistics of the number of cases of phthisis which occur in families apparently as the result of transmission from one individual to another, and those in which the disease is believed to have been derived from milk. But the practical difficulty would come to be this—unless you were going to isolate all phthisical patients—you could not, during life, distinguish the difference between tuberculosis when it comes from cows and when it is caused by infection from man to man. Now, I want to know—supposing that you do stop all infection from cows, will you thereby do much towards stopping consumption in man? Suppose you were to destroy all these tubercular cows and cut off the milk, would you diminish the mortality from phthisis unless you isolated or destroyed your phthisical patients and disinfected all their sputa? That is a point which must not be lost sight of.

Dr James—Mr President, it would be an act of cruelty if I were to inflict any lengthy remarks upon the Society at this hour; but I would like to say something as regards the question which Principal Walley has brought into such prominence, and which has been referred to by Dr Greenfield, namely, the transmissibility of tuberculosis from animals to man, and the best way by which tuberculosis in man can be benefited. Well, of course we know that tuberculosis can readily be transmitted from animals to animals, and we conclude that the same thing happens as regards man. We know that the administration of sputum—vaporized and allowed to be inhaled or ingested with the food—will sometimes produce tuberculosis, and we know that the bacillus, if administered to animals, will produce this disease; but the question is, In our efforts to prevent consumption, what can be done by stamping out? Now, I for one, as the result of my study, believe that the stamping out is not of the very great importance which some attach to it. Supposing we take the disease tuberculosis as it affects man. If you divide the life of a man into, say, seventy years, you find that brain tuberculosis is most common about four, that intestinal tubercle is most common about eight or ten, and that lung tuberculosis reaches its maximum about thirty. Well, now, take brain tuberculosis. If it is due entirely to a germ in the atmosphere, that germ must be inhaled, and one would suppose it would go first to the lungs. If it is due to milk, one would have expected that it would have been in the intestine. It is specially in the period of infancy that tubercular milk may be taken, and yet at that period it is the brain rather than the intestine and lungs which is affected. In different animals, if I understood Principal Walley aright, tuberculosis is commonest in the bovine species; but in horses, I think, it is not so common. Well, Principal Walley thinks this is due to a peculiarity in the stomach; but in thinking over this matter, I am of opinion that it is rather due to the different lives led by these different animals. The cows are kept in the byre and the horses get plenty of exercise. The conclusion that I draw from the whole matter is, that the germ is practically ubiquitous, and when the question is put before me, Are we to benefit our fellow-mortals by attempts to stamp out tuberculosis just as we can stamp out small-pox or scarlet fever? I am inclined to say that we cannot expect much in this way. I believe, as the result of my study of phthisis in man, that if you killed every tubercular animal in the country and banished every phthisical patient, you would have as bad a set of conditions subsisting in another six months. You must deal with tuberculosis, not so much by stamping it out, as with rabies or scarlatina, but as with insanity or cancer, by improving the general conditions. The practical point, therefore—and I was very glad to hear Dr Littlejohn's remark, as we can safely do it in the case of animals—is to stamp it out to a certain extent; but to remember that our efforts would altogether fail if we did not pay more attention to the sanitary conditions of the places where they keep those animals. I believe if the byres were better looked after, and if cows, instead of being tied up in byres all day, were taken out for exercise like horses, we would be doing more to get rid of the mischief than by attempts to stamp it out, because, as I said before, the bacillus is practically ubiquitous.

Dr Cathcart—I would like to ask one or two questions. In the first place, it has been pointed out that considerable difference exists between the tuberculosis in man and in animals, especially in regard to the organs which have been affected, and Dr James has offered one suggestion as to the reason attending the difference between horses and cattle. I should like to ask him if there may not be another reason; if the food of the animals may not make some difference? Man feeds upon food which has been cooked. Now the oxen and horses do not feed upon cooked materials. Oxen feed upon soft stuff generally in their domesticated condition—such fresh matter as grass, where organisms would be more frequently found than in the dry materials forming the principal food of horses. If Dr Woodhead's view is correct as to the children, would it not appear that they did get the infection through the alimentary canal, in view of the much greater frequency of the disease in the liver, in the spleen, and in the abdominal organs; whereas, in man, it is so much more frequently found in the lungs? Suggestions have been made as to how the dairies may be inspected. Would it not be possible, without altering the character of the milk, to affect it in such a way that it would not be dangerous? It is said that by raising the temperature you may destroy tubercle bacilli. Would it not be possible to do this without affecting the quality of the milk? If so, we would have a simple way of dealing with tuberculosis from another point of view. Just another question. In reference to what Dr Littlejohn has suggested, may not the fact that phthisis has not gone on increasing be accounted for by this, that we have improved our sanitary conditions? At the same time the general danger of tuberculosis may have increased. It would be important to answer the question whether or not there are other manifestations of tubercular mischief which have increased although phthisis has not done so. It may be that the atmosphere of this town is not such as would likely propagate lung affections.

Dr Peel Ritchie—We have heard many observations upon the different views held as to the pathology of this question, but there is a more practical question which I should like the Society to consider, and that is, Whether we should not take up those two points which Principal Walley has mentioned, by which we might aid in putting a stop, so far as it is possible, to the dissemination of disease through milk and flesh; that is, by a more thorough investigation of dairies, and also by application to the Privy Council for further powers? I would therefore beg to submit to the Society that they remit to the Council the consideration of these two points, and that the Council report again to the Society whether we should not, as a Society, address the Privy Council in aid of those views which Principal Walley has brought forward. I think it would be of very great importance. It has also occurred to me in visiting the dairies that there is something very far wrong. I cannot say that I think the veterinary profession is altogether blameless in the matter. One thing has often struck me as being overlooked, and that is, the treatment of a cow after calving, and I think that if a little more attention were paid to the cleansing of the animals,—if they were better looked after and allowed to get the air, instead of being contaminated by the sometimes fetid smells which I have noted in byres, it would be very much to be desired. Another point, also, which I think may help in the production of tuberculosis amongst these animals is this, that they are sometimes kept there as long as they will possibly give a fair supply of milk, and it is vexing to see the reduced condition of the animal before it is taken from the byre. The artificial feeding is also objectionable. I think there are various points in the dairy question which call for investigation, and it would be well, therefore, that we, as a Society, should aid Principal Walley in the views which he has put before us.

Mr A. G. Miller—I have much pleasure in seconding what Dr Ritchie has suggested, namely, that the propositions of Principal Walley should be referred to the Council of this Society, and that the Council should report to a future meeting of the Society. Whilst I am standing here I would like to ask Principal Walley whether he has employed iodoform in the treatment of tuberculosis in the lower animals, and if so, what effect he has noticed produced by it? I found iodoform wonderfully beneficial, and I would like to know if he has used it, and if so, to what effect? Another point has occurred to me. When the flesh and entrails are condemned of any animal suffering from tuberculosis, what is done with them? Would it not be possible to have them burned, seeing that a high temperature would probably be the most effectual way of disposing of the tubercular bacillus. Lastly, I do not agree with Dr James and Dr Greenfield in thinking that because we cannot altogether stamp out tuberculosis in the human subject, therefore we should not do anything. We should diminish it in any way we possibly can. As the father of a large small family, I most undoubtedly feel uncomfortable after what I have heard to-night. I cannot say that I have listened to Principal Walley's paper with pleasure. I certainly have listened with very great attention—and profit, I hope; but it certainly is a most unpleasant paper to listen to when one has got a number of children at home, who are every morning taking milk along with their porridge and every night taking it in tumblerfuls. I would therefore like to ask this question, Is there any way by which we could recognise the milk from a tuberculous udder?

Principal Walley—Mr President, I have listened with very great pleasure to the discussion, but I must confess that I think it is a great pity it should have been curtailed by the lateness of the hour. So far as it has gone, Dr Littlejohn made one or two remarks that I think require a little notice. He brings forward statistics to prove a certain thing, and then demolishes his own statistics by calling them untrustworthy, because, as he says, doctors will not always say what the disease is from which their patients suffer. He further says that phthisis has not increased within the last few years. If phthisis is due to the ingestion of milk from cows or to the eating of flesh, it is a point, I think, in my favour, for the simple reason that it shows that there is a very different state of the dairies and the inhabitants of the dairies at the present time than that which existed some years ago. If Dr Littlejohn knew the dairies as I do, he would find there is a vast difference in them during the past few years, although, I must confess, they are not by any means what I would like. We have difficulties, however, to contend with of which outsiders can have no conception, and one of the greatest of these is to get people to keep their ventilators open. They are bound to have the ventilators, but they will persist in closing them. You may go into a byre when the odour is perfectly sickening, and throw the ventilators open, and on going back the next day find them again closed. They say to you that the animals won't milk unless they are kept warm, and unless some one is standing by all the time, the ventilators will be kept closed. I may say that the walls of the very best byre in Edinburgh, which is perfectly well known to Dr Woodhead and Professor M'Fadyean, is generally streaming with the watery vapour from the expired air of the cows, and yet it could be ventilated in a way that no one could take exception to. Dr Littlejohn has also referred to the sewage. No sewage is put on the meadows until immediately after the grass is removed. Probably it may be put on again a week after that, but no further sewage is put on until the grass is again cut. Now it will depend entirely on the season how long it takes the grass to grow. If a good season the grass grows rapidly, and it may probably be removed in a month, but the sewage matter is in many instances washed deep into the earth by rain, and if it is not wet it may be blown away by the wind. The grass certainly will be contaminated to some extent; and in several instances I have seen cases of dysentery from the effects of sewage grass. But that is a matter I cannot interfere with. People will pay very high prices for the grass (so high that it is simply astonishing), so that it is no use attempting to do anything unless there is a law passed that they must not use it. As to the danger of infection from rabbits, my experience is that tuberculosis is most largely found in the liver and sometimes in the intestines; but in Scotland, at any rate, it is not the practice to eat the liver—so much is this the case that I never can get a cook to preserve the livers of either rabbits or poultry, fond as I am of them. It is generally said that the cat has run off with them. I think most people throw away the liver. Nor do people eat rabbits when reduced to the miserable condition usually consequent upon tuberculosis. Rabbits so affected are generally very lean and emaciated. There is another thing which has been spoken to by Dr Cathcart in reference to inspection, *i.e.*, why we don't have the affected animals destroyed. We have positively no legal power to do so. Much has been said, also, in reference to bovine tuberculosis being *produced* by the insanitary state of the byres in large cities. Let it be distinctly understood that insanitary conditions in themselves have no power to *produce* the disease; they may favour its spread, and, when present in the system of an animal, may aggravate it or hasten its progress; but in the vast majority of cases, the disease exists in the system of the cows when they are purchased. Only the other day I was asked to examine two cows (one of which had been purchased a fortnight, the other a month), which were supposed to be suffering from pleuro-pneumonia; I found one dying and the other dangerously ill from tuberculosis—the carcass of one of these cows (for which upwards of £20 had been paid a few weeks before) was seen by Dr Littlejohn. As to the little matter at Carlisle, the doctor has made a mistake. We went there intending to give evidence, but we never said a word. We were not asked. The case was settled without any question being asked in reference to it; but he is quite right as to the facts. Dr Greenfield made some remarks in reference to the veterinary profession, which, I think, are a little unjust; and I have only to state that we are not so backward as the *Lancet* and some medical men seem to think we are in these matters. I note what he has said as to the instances of tuberculosis in the dog. I must confess I have not seen any cases that I could positively say were tuberculosis. In a few cases I certainly have seen lesions closely resembling those of tubercle in this animal, but you must remember that it is only very recently that we have had any positive basis upon which to found an accurate opinion as to what is and what is not tuberculosis. Dr Greenfield asked if calves had been inoculated with tubercle. I think I am more than safe in answering that question in the affirmative. I believe there is no animal which has not been inoculated with human tuberculosis. I have here books which simply teem with records of experiments, by Continental professors most largely, in reference to this point, and calves have been experimented on more than any other animals. Dr Greenfield also makes a remark about affected cows in byres. I may just say that if I were to take out, with the permission of the magistrates, all tuberculous cows, and compensation were given to the owners and the flesh destroyed, as Dr Littlejohn has said, it would be another £800 out of the pockets of the ratepayers. I cannot agree with what Dr James has said. He made some remarks with reference to the difference in age, and as to whether the difference in the temperature might not influence the production of tuberculosis. I think there would be a higher temperature in children than in animals. Dr James refers also to the fact that it appears in the lower part of the alimentary canal. I think there is no difficulty in explaining that. We all know that the tubercle bacillus must, in order to develop, get rest; that is looked upon as one of the principal things in reference to its development. It certainly gets no rest in the upper part of the alimentary canal, or at least not nearly as much as it gets in the lower portion. The bacilli are exposed to the action of the gastric juice in the upper part, and I think it is only reasonable to assume that many of the bacilli may be destroyed by the gastric juice, and many others would be so weakened that they would be incapable of producing harm. Of course they may regain some of their lost strength in the large bowel, and then produce harm, but I will not enter into that question. Another question is about the difference in the case of horses not being tied up like cows in byres. That, I think, does not hold good in any case in country districts. Cattle are not tied up in country districts as they are in towns; and yet most of the cows that suffer from tuberculosis in towns have it in their systems before they come from the country districts, and in these districts, of course, they would have plenty of exercise. Dr Cathcart asks if food may not have something to do with it, seeing that cows eat more soft material. Probably there is something in that, but horses are also fed upon the same food. I cannot, of course, explain why the horse is not so liable to tuberculosis as the cow, except, perhaps, that the gastric apparatus of the latter is more favourable to the development of the bacilli than is that of the former. We know that the horse is not generally so subject to contagious diseases as the ox, if we except glanders.

Of course I do not deny that the method of feeding and the difference in the digestive organs may have something to do with it. Dr Ritchie has made a remark in reference to the condition of cows after calving. How are we to deal with these cases? Dr Ritchie is perfectly right. The stench is frequently enough to kill all the cows in the byre, but if I advise the removal of such a cow, I am met with the statement that there is no other place in which to put the animal; and even although you order the animal to be removed, when you go back you find it still there. Dr Ritchie also made a remark as to the animals being kept in the byres as long as they will give milk. That is not the case. The cows in this city are only kept so long as they give milk profitably. As a rule you do not find cows kept more than twelve months, and in some cases only nine months. They go away the moment they cease to be profitable. They are not kept until they absolutely cease to give milk. The gentleman who spoke last, Dr Miller, asks what becomes of the viscera of tuberculous cows. I can ease his mind on that point. It is all burned. I have no power, of course, to superintend the slaughter-house, but it is understood that the viscera is all burned, or if not actually burned, that it is put into a huge tank and destroyed by steam, or it is acted on by the steam, so that everything in any way dangerous is destroyed. Dr Miller also asks if you can recognise milk from a tuberculous udder. I answer—No, except by the aid of a microscope. If the animal is very bad, the milk is of a poor quality, but there is nothing else about it of which you can take cognizance.

Meeting V.—March 7, 1888.

Dr John Smith, *President, in the Chair.*

I. Election of Ordinary Member.

Thomas Russell, L.F.P.S., Davidson's Mains, was elected an Ordinary Member of the Society.

II. Exhibition of Patient.

Mr Cathcart showed a case of colles' fracture produced by a blow directly backwards on the palm of the hand from a lever. Occurring in this way the fracture must have taken place by the breaking strain conveyed by the ligaments to the bone. The case therefore confirmed the view he d by many, that the injury might, if it did not always, happen in this way. Another point of interest was that while the usual backward displacement was well marked, the lateral displacement of the hand to the radial side was absent, there being, if anything, a displacement towards the ulna instead.

This seemed to confirm the view that the lateral displacement was due to the direction of the force, seeing that the accident generally happened by a person falling on the hand, and thus causing the bones of the forearm to be pushed to the ulnar side of the hand, *i.e.*, displacing the hand towards the radial side of the forearm.

III. Demonstrations.

1. *Mr John Duncan* demonstrated—(1.) A method of applying SYPHON EXHAUSTION to cases of empyema. It consisted essentially of a large flat piece of plain rubber, a long rubber tube, and an air-tight pliable bag. The cavity was filled with warm boric lotion. The tube, also filled, was inserted into the cavity through a hole in the plain rubber cut smaller than itself, and an elastic bandage passed round the chest to keep the rubber in position. The air-tight bag was then filled, and by pressure at the bottom half emptied again. While the bag was brimming over the tube passed through its screw stopper was inserted into it and the stopper screwed home before the pressure was relaxed. The patient could walk about with the bag in his pocket. In one case in which this method had been tried it had worked very satisfactorily. When first commenced the capacity of the empyema cavity was thirty ounces. In one month it had diminished to a capacity of seven ounces under pressure (when the syphon action was reversed), five ounces when the bag was held on the same level with the chest, and two and a half ounces when the bag was lowered. After that the patient was allowed to have it under his own control, but by neglecting to adjust the bag properly when inserting the tube he allowed air to enter, put a stop to the syphon action, and enlarged the cavity, which was again reduced under proper treatment, and now contained only half an ounce when the syphon was acting, five ounces when reversed. (2.) A method of applying CAPILLARY DRAINAGE for the removal of thin fluids. This consisted in the use of threads of worsted which might be passed through a wide rubber tube. It had proved of great service in a case of abscesses of the thigh and abdomen connected with hip-disease. The abdominal abscess had opened into the ureter, and the whole of the secretion of one kidney pouring through it for a time kept the bed constantly wet and the patient very uncomfortable. The urine was drained off by threads of worsted passing through a wide rubber tube which was passed deeply into the abscess and carried over the side of the bed to a vessel, into which the urine dripped at the rate of nearly thirty drops per minute. This method might be of use in all cases where the discharge was thin.

2. *Prof. Chiene* showed an uncontracted BLOOD-CLOT. It was obtained from a horse killed in the slaughter-house on October 28th, 1887. He had only once before seen a similar clot, and it was in the possession of Sir Joseph Lister.

Mr Cathcart said he had twice got blood from sheep killed in the slaughter-house which showed a similar condition.

IV. Exhibition of Pathological Specimen.

Dr A. Bruce showed a specimen and drawing of DIPHTHERITIC MEMBRANE of bladder and urethra. The patient had at one period of his life been operated on for stricture, and died after an attack of retention. The catheter was easily passed. After it was withdrawn some bleeding followed. At the post-mortem examination there was found a slight septic pyelitis of the kidneys. In the bladder there was a firm, dirty-yellow membrane adherent to the mucous membrane underneath. This extended along the urethra almost to the meatus. The mucous membrane appeared to be ulcerated.

V. Original Communication.

THE CLINICAL VALUE OF TEMPERATURE OBSERVATIONS IN SOME ACUTE AND CHRONIC DISEASES.

By J. O. Affleck, M.D., F.R.C.P. Ed., Physician to the Royal Infirmary, Edinburgh; Lecturer on Practice of Physic and on Clinical Medicine, Edinburgh Medical School; Examiner in Clinical Medicine in the University of Edinburgh.

Among the many important additions to our means of accurate investigation of disease which have marked the past few decades, there is none which excels in value the clinical thermometer. By it we have been enabled to distinguish, from an early period in their development, not a few of the more serious and common of the acute maladies; to trace their course, and recognise deviations from their ordinary progress; to detect the advent of complications, to prognosticate their issues, and to concert measures for combating or effectually treating some of their most urgent symptoms—often to the saving of lives in imminent danger.

Those who are comparatively young in the profession regard this instrument as an essential part of the equipment of the physician, and are apt to wonder how their predecessors worked so well without its aid; while those who are old enough to have practised when no such thing existed will freely admit that by its possession they are placed in a far better position for obtaining correct and helpful information than they were before. Great, however, as has been the boon conferred alike upon physician and patient, it may be fairly questioned whether it has been utilized to the extent to which it is capable of serving its important purpose—whether we obtain from it all the information which as an instrument of scientific accuracy it is able to afford us. It is rather to be feared that most of us who carry about this little tool and readily acknowledge its value are but too apt to make use of it, if not in a perfunctory manner, at least with but an in-adequate sense of its high clinical importance, and that thereby much information which might be turned to practical account is lost to us or never reaches us.

It is with this conviction, and because the subject is one which has occupied my mind in connexion with opportunities I have had or have taken for attentively observing temperatures in various diseases acute and chronic, and especially because I believe that the directing the notice of my younger brethren in particular to this matter may prove useful to them in their practice, that I venture to occupy the time of the Society for a little to-night.

But at the very opening I must submit an explanatory or apologetic preamble. In the first place, I must claim the indulgence of the senior members of the profession present for bringing forward, as I know I must do, facts which are not only not new to them, but regarding which they might perhaps be able to teach me much. It is the younger practitioners I have had specially in view on the present occasion, and my single desire is to bring under their notice observations I have made, and lessons I have learnt, which have been and constantly are helpful to me in practice; not asking their acceptance of these at my hand as absolute facts, but rather inviting their attention to them with the view of their subjecting them to proof when occasion offers, and of securing their interest in a clinical subject which, as already stated, there is reason to believe is but inadequately studied. Further, I must clearly guard myself against being supposed to give undue prominence to one symptom as an infallible means of recognising or tracing the progress of any disease. On the contrary, I hold most strongly that such a symptom as the temperature cannot be dissociated from other symptoms of equal or greater importance which together make up the portraiture of any malady, especially any acute malady. All that is contended for is that the course of the temperature in the case of not a few diseases is capable of indicating, with at least approximative accuracy, not only the chief events in their clinical history, but those departures from their wonted progress which may arise and influence in a marked degree the issue of the case. Into the subject of the physiology of the bodily temperature I cannot of course enter here, interesting and tempting though it be, nor even into its general pathology, much as I should have wished to have discussed questions relating to the morbid deviations both in the upward and downward directions from the normal heat. Both of these topics, and also those of the best methods of making temperature observations and the treatment of high temperatures, are excluded from the scope of this paper, which simply professes to record clinical observations made and deductions drawn which these appear to justify.

It will be sufficient, in the way of general remark, to recall to mind the recognised facts that any material alteration in the course of the bodily temperature bespeaks not merely the existence of some morbid process at work in the body, but implies in an especial manner a waste of tissue should such alteration be of the character of fever, or a lowering of vital action should the thermometer indicate a fall from the natural warmth. Both of these conditions also include in them serious disturbance of that process of heat regulation which is so full of interest in its adaptiveness, and involve likewise changes affecting the functions of vital organs as well as important modifications of secretory and nervous influences, which are in constant operation in living bodies. There are probably instances of forms of disease where the *primum mobile* is none other than the disturbance of this heat function. Such is the view held by some as to the etiology of acute rheumatism, and in the case of those maladies which arise as the result of exposure to either extreme of the temperature of the atmosphere, such as sunstroke on the one hand, and death by cold on the other. Nevertheless, a heightened or a depressed temperature is in most cases symptomatic of some recognisable lesion or disease process already at work before such temperature-change arose, and its value as a symptom, particularly in acute maladies, consists largely in its quick responsiveness to morbid action, so that it oftentimes, for instance, precedes pain, dyspnœa, etc., in the onset of inflammatory maladies, and during their progress announces impending dangers, registers complications, and forecasts results, all with an accuracy which no subjective symptom can be trusted to reveal.

It is clear, however, that such observations, to be reliable, must be made with sufficient care and frequency to ensure their expressing, as far as possible, the continuity of the phenomena represented in any given case. Thus the conventional method of taking temperatures morning and evening, although convenient, is in many instances misleading, and fails to throw light upon conditions which a more frequent observation would have fully explained. It may be urged that such precise observations can only be satisfactorily carried out in hospital practice, and are impracticable otherwise. Now, however, when the clinical thermometer is finding its way into families for use by intelligent parents, nurses, and others, and when it is remembered that the method of temperature taking can be easily taught to such, any difficulty of the kind referred to can be readily surmounted.

In submitting the following statements as to the clinical value of temperature observations in certain diseases, it is right to explain that the facts narrated have been almost without exception derived from cases under my own care, chiefly in hospital work, and that the illustrative charts representing, for convenience, for the most part only morning and evening temperatures (although, in point of fact, in many of the cases four-hourly observations had been made), are the bases upon which the views to be indicated rest.

It is more than likely that many or most of these views have already been expressed by others, and if so, they will merely act the part of confirmatory evidence—which, however, is not wholly to be despised—while, on the other hand, it is possible that some points may be brought out which have hitherto attracted but little notice. In any case the observations have been made irrespective of any existing views or doctrines. It must always be remembered that exceptions, more or less numerous, to general statements such as those here set forth—even granting that these are in the main correct—must be expected to be encountered, since individual peculiarities, complications, and other modifying influences enter here as they do in all other conditions of disease. If, however, taking all such circumstances into account, it is yet possible, as I believe it to be, to formulate conclusions which possess real value to the physician in practice, it seems worth while to make use of facts and observations which may contribute towards such a result.

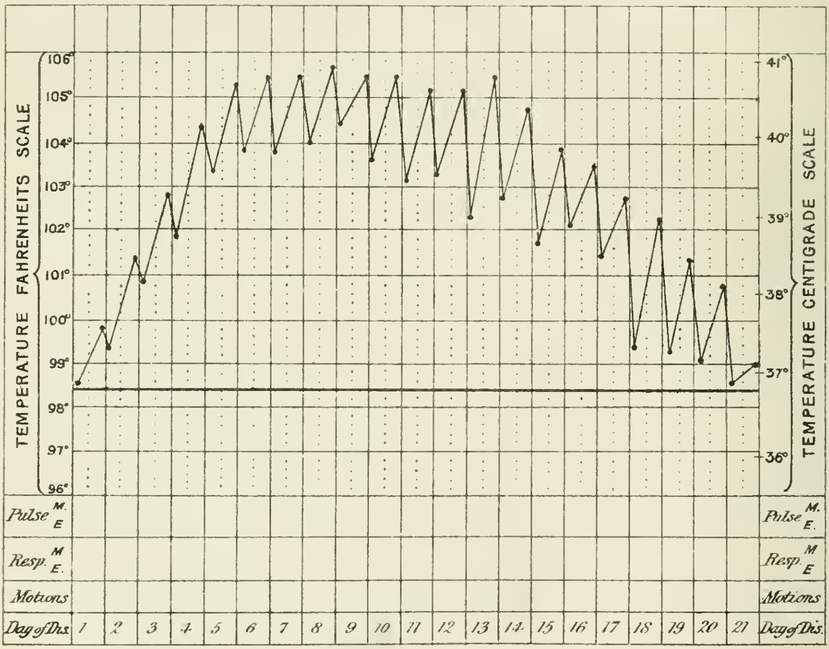
In first directing attention to temperature teachings in ACUTE DISEASES, I shall have to restrict myself to a few of the more common and important ailments where the temperature is confessedly a symptom of high value diagnostically and otherwise.

Typhoid Fever is one of these, and the light which the thermometer is capable of shedding upon the whole clinical history of this disease entitles it to be first considered. The temperature phenomena of this fever, pursuing its normal course, are roughly represented by a crescentic curve, and its duration is given as about three weeks. (See Chart, Fig. 1.) The type of fever is on the whole remittent. As a matter of fact, however, typical examples of this disease are not very often met with, and in all the stages of its progress it will be found that departures from the described character of the temperature are very frequent and often very marked. Thus it occasionally happens that the onset of the symptoms is comparatively sudden, and that the acme of the temperature is attained early. Again, how often one sees the temperature of the second week continued through the third and even fourth, so that comparatively few cases run their course under twenty-eight days, and a large number exceed that. This will be the less surprising if we reflect that in this fever the temperature appears to stand in very close symptomatic relationship with the progress of the bowel lesion. In protracted cases of typhoid with high evening temperature, or with a temperature almost uniformly high, say over 103° F., we may infer with tolerable certainty not only the existence of extensive ulceration, but probably also new glands or areas of the mucous membrane continuing to be attacked. It has often occurred to me to observe this in post-mortem examinations of such cases, where all stages in the ulcerative process were represented in the affected portion of the intestine; and the relapse so frequently observed in this fever (of which the temperature so usefully admonishes us) is undoubtedly associated with the recrudescence of the local lesion in the bowels.

Fig. 1

Typical Case

Name *Age* *Disease Enteric Fever* *Result*



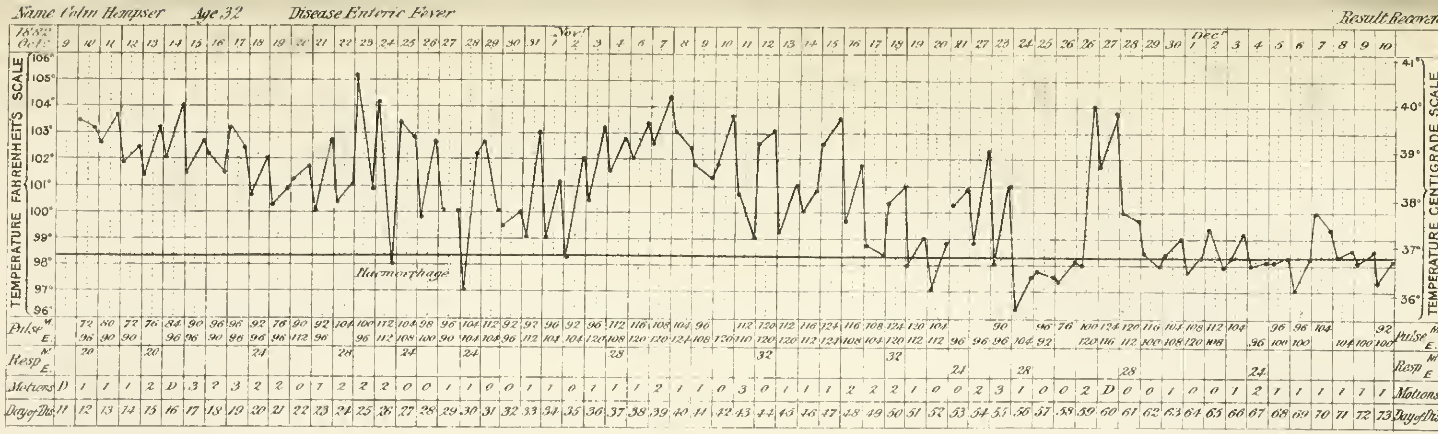
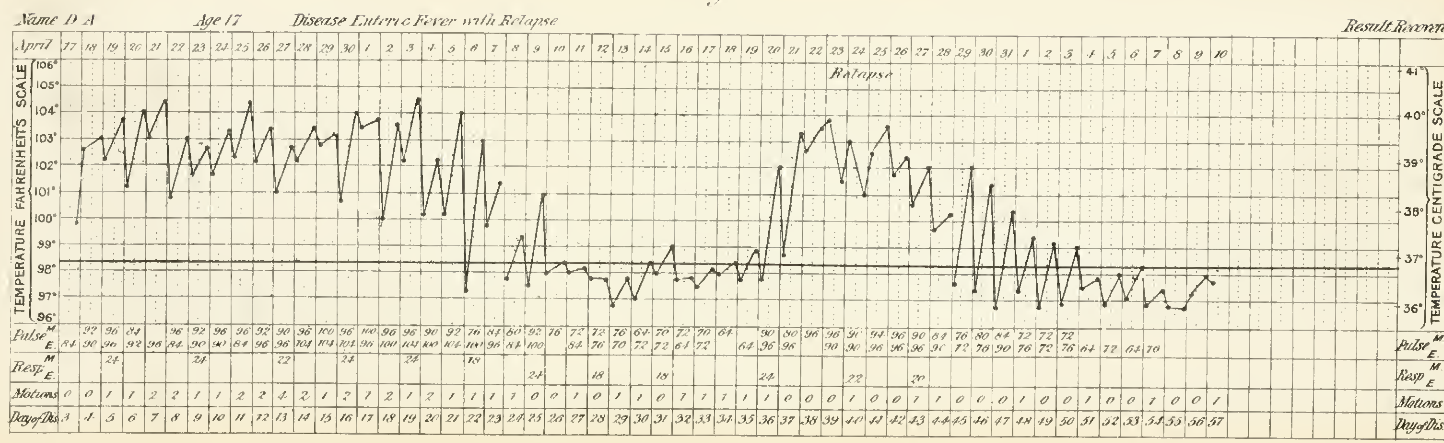

Name Colm Hempser
Age 32
Disease Enteric Fever
Result Recovered
TEMPERATURE FAHRENHEIT'S SCALE
TEMPERATURE CENTIGRADE SCALE
Haemorrhage
Pulse
Resp
Motions
Day of Dis.

Fig. 3


Name D A
Age 17
Disease Enteric Fever with Relapse
Result Recovered
TEMPERATURE FAHRENHEIT'S SCALE
TEMPERATURE CENTIGRADE SCALE
Relapse
Pulse
Resp
Motions
Day of Dis.

The relation of temperature and pulse in typhoid is very interesting, and in not a few instances renders valuable aid in diagnosis. In cases of moderate severity, or mild cases, where presumably there is little bowel lesion, or where this has not passed on to extensive ulceration, it will frequently, if not, indeed, usually be found that the pulse-rate is low compared with the height of the temperature. Thus I have often seen a temperature of 104° with a pulse not exceeding 80 ; and a case ran through all its stages where the pulse-rate never attained 100. Such cases are mostly favourable, with comparatively quick recoveries. This slow pulse is often singularly useful in diagnosis in the early stage of typhoid, where the high temperature and other symptoms might suggest some other disease. Where the pulse is rapid all through, or, having been slow at first, becomes accelerated later on with persisting high temperature, the case, making allowance for nervous or constitutional irritability, is probably one of considerable severity. It is also to be noted that sometimes the pulse increases in rate after defervescence from heart irritability and weakness, a condition in which digitalis often proves of eminent service. This temperature and pulse relationship is noteworthy and important from the second week onwards, inasmuch as most of the complications so common in this fever are capable of ready recognition by a careful plan of temperature observation. Changes of sudden and alarming character may be brought about by a hyperpyretic movement of the temperature, of which the early discovery will give opportunity for interfering actively, and sometimes successfully. But even more frequently still may arise during this time one or other of those accidents to which the typhoid patient is exposed, and of which the temperature affords one means, although, of course, not the only one, of quick detection.

Thus a hæmorrhage may often make itself known by the rapid fall of the temperature, together with pallor of the face, sighing respiration, and small, quick pulse, even before the blood has appeared in the stools, and suggest the prompt administration of styptic remedies. (See Chart, Fig. 2.) No doubt a similar set of symptoms may result from perforation, and in many instances it is scarcely possible to distinguish between the two occurrences; yet in the latter the abdominal pain, vomiting, and supervention of peritonitis are specially marked. A condition occasionally arises resembling those just described, but owning a different cause from either, and yet equally dangerous, viz., where in a case pursuing its normal course the temperature suddenly drops from a high degree, and signs of sinking show themselves, without any evidence of local cause or even much disturbance in the patient's subjective sensations. This is simply collapse from heart failure, and it is early recognised by the descent of the temperature. Its detection in this way is all-important with the view of averting by prompt stimulation the otherwise frequently fatal result. Other complications, more or less common, owe their discovery to carefully observed temperature changes.

It is important to know that in any case of collapse, or other change where life is not quickly extinguished, the temperature generally rebounds upwards again, often to a higher point than that at which it stood before, and that from this time onwards the temperature range becomes more irregular and aside from the typical characters of the fever. (See Chart, Fig. 2.)

The thermometer indicates the departure of the fever, which is generally held to be at an end when the evening temperature over several days continues normal. But evening observations must be made from time to time during convalescence, especially after any change of diet, and in connexion with movement from bed, etc., for it is by the continuance of this practice mainly that the relapses so common and so important in this fever are detected. Into the causes of these relapses it is not possible at present to enter, but they may arise at a time when all risk of such things seemed at an end; and while no doubt they are usually of short duration and have a favourable issue, they may, on the other hand, extend over as long a period as the original attack, and they occasionally terminate fatally. (See Chart, Fig. 3.)

Certain anomalies of the temperature in typhoid are worthy of notice; for although wholly exceptional, they show that important as is the thermometer in the diagnosis of this disease, it would never do to trust to it alone. Thus there have been instances of typhoid, even in epidemic outbreaks, such as that described by Struve (*Berlin. Klin. Wochenschrift*, 1871, No. 30), as occurring in the German army at the siege of Paris in 1870, where the temperature throughout was at normal or subnormal point. In such instances it has been justly held that peculiar environments or conditions of life must have modified some at least of the features of the disease. That such is possible is, I think, illustrated in one of my cases of which a chart is before you (see Chart, Fig. 4), which occurred in the person of a night nurse in the Royal Infirmary, who was attacked with typhoid and treated in the Fever Hospital. It will be noticed that for many days the temperature showed a contrary course to that ordinarily manifested, inasmuch as there was a regular morning rise and an evening fall, but that ultimately the case assumed the normal character as regards temperature. This is not the only instance of this kind of inverse temperature in typhoid which I have observed, although, on the other hand, I have had night nurses under my care in whom the temperature pursued the ordinary course.

The co-existence or recent occurrence of any serious disease may also markedly modify the character of the temperature symptom of typhoid. In children the type of the fever is markedly remittent, with frequently very high evening temperatures; while in persons over middle life the height of the temperature is often not

Fig 4.

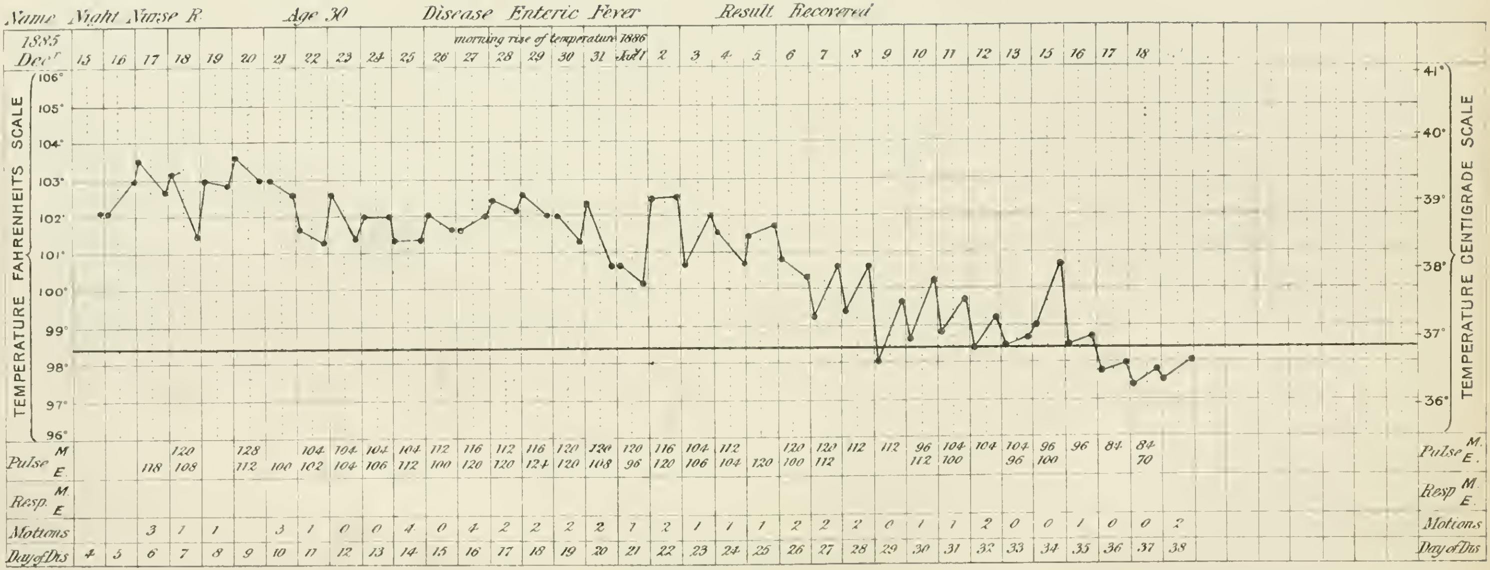
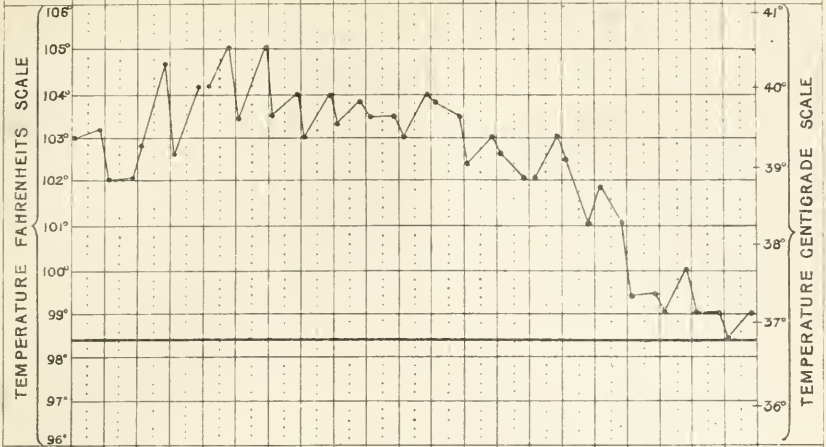

Name Night Nurse R.
Age 30
Disease Enteric Fever
Result Recovered
1885 Decr
morning rise of temperature 1886
Jan 1
TEMPERATURE FAHRENHEITS SCALE
TEMPERATURE CENTIGRADE SCALE
Pulse M E.
Resp. M E
Motions
Day of Dis

Fig. 5.

Name T.W *Age* 23 *Disease* Typhus Fever *Result* Recovered

July	16	17	18	19	20	21	22	23	24	25	26	27	28	29	30	31	1	2	3	4	5	

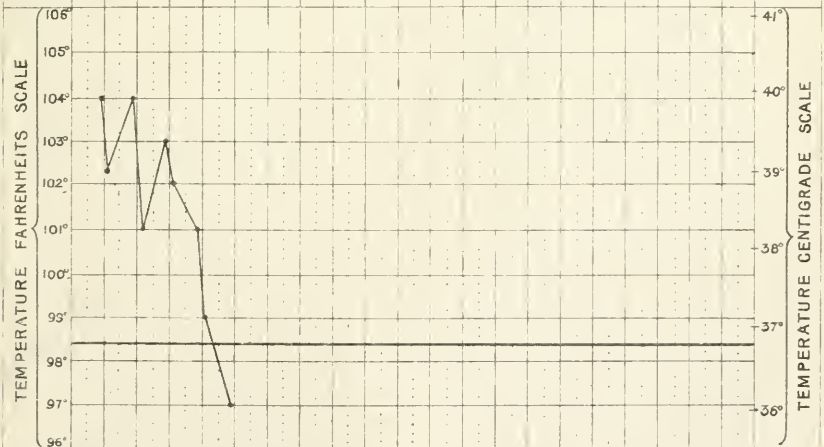


	16	17	18	19	20	21	22	23	24	25	26	27	28	29	30	31	1	2	3	4	5	
Pulse M.		100	108	112	120	112	112	120	120	122	120	112	112	112	112	100	100	92	90	90	84	Pulse M.
Pulse E.	120	104	112	116	112	120	108	120	120	116	116	120	108	100	100	104	96	84	72	80	90	Pulse E.
Resp. M.			24				24				24				24							Resp M.
Resp. E.			20				24				24				22							Resp E
Motions	1	0	0	1	1	0	1	1	1	2	3	4	2	1	1	0	1	1	0			Motions
Day of Dis.	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	Day of Dis.

Fig 6.

Name E.L *Age* 50 *Disease* Typhus Fever *Result* Died

June	5	6	7	8	9



	5	6	7	8	9	
Pulse M			108	112	120	Pulse M.
Pulse E	104	112	112	120	124	Pulse E
Resp M			28	30	42	Resp M
Resp E.	28	32	34	36	48	Resp E
Motions	0	0	2	0	0	Motions
Day of Dis.	9	10	11	12	13	Day of Dis.

great, and so less to be trusted as the measure of the severity of a case.

The length of time a case of typhoid may sometimes continue is surprising. I possess charts showing a continuous febrile temperature for over 100 days before defervescence occurred, and where yet complete recovery resulted. No doubt long-continuing unhealed ulcers of the bowels explain such cases; but were it not for a careful observation of the temperature among other symptoms, one might very easily be led to suspect that an error in diagnosis had been made at the first, and that the case was really one of phthisis.

It scarcely falls within the intention of this paper to refer to the effects of treatment upon the temperature of typhoid. One remark only I can make, namely, that while I have not had sufficient experience of the cold-bath treatment of the fever to state any facts of importance, I have used extensively antipyretic drugs, and have found that, while they doubtless possess much value in some cases by breaking in upon the continuity of a long-lasting and very high temperature, they appear to have no effect in shortening the duration of the disease, but, according to my experience, seem rather to tend to prolong its course. Moreover, great care is necessary in their administration, particularly in large doses (say 20 to 30 grains of quinine), which I have more than once seen induce a fall of temperature and of pulse so great and so rapid as to amount to dangerous collapse, necessitating the prompt resort to stimulants.

Upon the subject of the temperature in others of the fevers it is not my purpose at present to enter at any length.

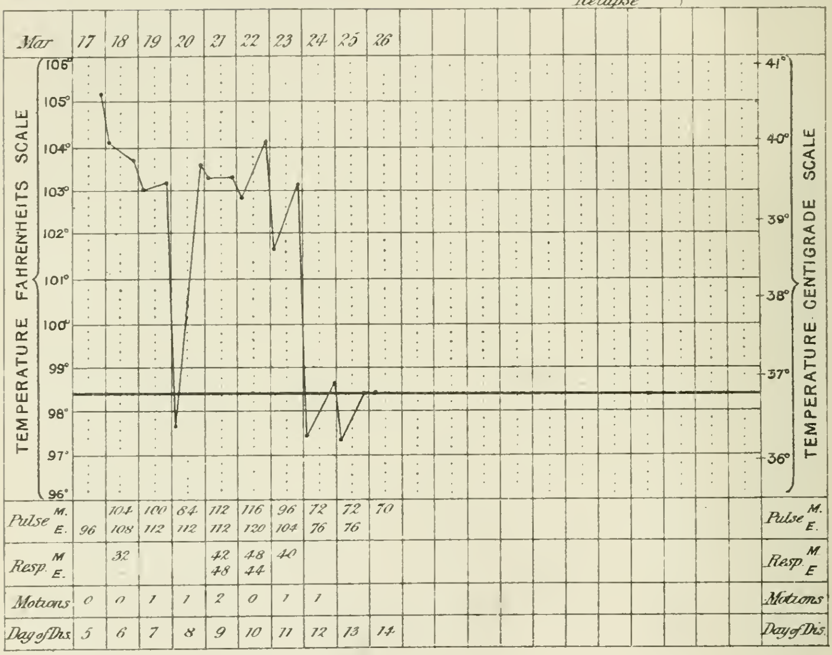
Typhus is now happily a rare fever in our country, yet it has not been wholly stamped out from our great centres of population. There are many interesting points to be noted in the temperature of typhus—its rapid development, its comparatively short acme, and its fluctuations prior to its critical fall—but only one or two can be here alluded to. Thus I have rarely found it to fail that the temperature in favourable cases attained its highest point about the end of the first week—that is, when the eruption had come fully out—and that, as pointed out by Dr G. Buchanan, should any notable change upwards in the temperature take place in the course of the second week, we might regard the case as an anxious one. From the eleventh day onwards the temperature becomes irregular, and epicritical falls of short duration mark its progress, and, other things being equal, are of rather favourable prognosis. The final fall takes place sometimes about the thirteenth day, more commonly the fourteenth, but it may be later by several days, in which case the defervescence rather approximates to that by lysis. (See Chart, Fig. 5.) It is extremely important in such a fever as this, just as we shall afterwards see in the case of pneumonia, to understand what is involved in a favourable crisis. Were we to suppose that the fall to normal of the temperature was all, we should be greatly mistaken. It tells of the crisis, but it does not by itself constitute the crisis. Unless every other disturbed function, notably that of the circulation, improves in proportion, we have a false or imperfect crisis, a condition fraught with danger. It is to be remembered, too, that the crisis may kill the patient (see Chart, Fig. 6), the shock of sudden cessation of febrile action being too great for the recoil of the system. The time of the crisis,—in which the thermometer is an helpful informant,—is therefore regarded with concern by every intelligent physician.

In the case of the *Exanthemata* the temperature, while yielding important information for many purposes, is less to be trusted to than is the case in the continued fevers. Indeed, in some points it is apt to lead us astray. Thus it is not always, although it is sometimes, a measure of the severity of a case; and examples of malignant forms of this class of fevers, such as measles, scarlet fever, smallpox, etc., may be often enough encountered where the temperature is by no means high. Again, such complications as post-scarlatinal nephritis may arise without any information from the thermometer. Notwithstanding we may yet in most cases profitably regard the temperature, and we shall find that it will repay our attention. In *Smallpox* it will help us to distinguish between the modified and the unmodified disease, and in general will give us a good idea of the severity of a case, and especially of the supervention of the more common and serious of the complications. Further, it may usefully guide us in treatment. During the epidemic in 1871 I encountered in a young girl, M. B., aged 14, a case which threatened to prove one of malignant type, in so far as that the temperature registered over 107°; there was profound unconsciousness with involuntary evacuations, a feeble and failing pulse, and a slight purplish appearance, merely suggestive of a commencing rash, upon the brow. I applied a wet pack for over an hour, when the whole aspect of the case became changed; consciousness returned, the temperature fell four degrees, a copious semi-confluent rash appeared, and the patient went through a severe attack, but quite recovered.

In the case of *Scarlet Fever* the temperature occasionally assumes at the outset a hyperpyretic point, with, it may be, urgent head symptoms. This, although alarming, is often of but short duration, and may in many cases be successfully dealt with by the wet pack. Again, it will often be noticed that there exists a certain relationship between the rash and the temperature, although too much cannot be founded upon this. It is rare, for instance, to see a very copious rash where the temperature is not considerably raised; but, on the other hand, the converse does not hold, since a very high, even hyperpyretic, temperature may be met with where little or no rash at all can be made out. In an ordinary average case the temperature has, as a rule, subsided by lysis at about the

Fig. 7.

Name J. F *Age* 23 *Disease* Pneumonia with Relapse *Result* Recovered



end of the first week. Where this has not occurred, but where it continues high, we shall probably find that the case is one of scarlatina anginosa, or that some complication—rheumatic, cardiac, renal, or other—has arisen.

In *Measles* the temperature suddenly rises with the onset of catarrhal symptoms, slightly remits before the rash appears, but quickly rises again with the eruption, which it then accompanies till the latter has attained its maximum, when it subsides with something like a crisis. Here the thermometer assuredly proves of value, for it will be found that the failure of the temperature to depart with the rash will almost certainly betoken the onset of a complication, most probably that of bronchitis or pneumonia.

Passing now to consider other acute maladies in which temperature observations furnish us with highly important information, I shall ask your attention briefly to the very common ailment *Croupous Pneumonia*, a disease which we all know is now regarded by not a few as a specific fever, and which certainly differs from every other inflammation in its remarkable self limitation and in many other ways. Doubtless cases differ in their whole clinical history, as we know they do in their severity, but speaking generally, the commencement is well marked with rigors and the rapid access of fever of high degree, the acme of which is soon reached and which is of the continued type, with such slight daily fluctuations as are consistent with this character of febrile action. All through an attack the thermometer teaches us much. Thus at the very onset temperature observation may suggest this disease even before any other symptom, and certainly before any physical sign has as yet with any distinctness given the hint. A rigor with sudden rise of temperature, vomiting, together with, it may be, uneasiness or pain in the side and slight acceleration of the rate of respiration, being more likely to be interpreted correctly by a diagnosis of pneumonia than in any other way. True, cases do occasionally begin, and sometimes those, too, which prove to be the worst cases, where the invasion is so insidious and indistinct, that the temperature is valueless in announcing them, but such instances are certainly exceptional. The progress during the week or so which marks the fastigium or height of the disease, offers some interesting and important facts which the temperature rather strongly expresses. Thus cases are sometimes met with where the whole symptoms suddenly abate after continuing for two or three days, and the disease, the presence of which was made out not simply by temperature observation, but even by recognisable physical signs, appears to abort. Again there are not unfrequently observed *pseudo-crises* (see Chart, Fig. 7), where after a few days the temperature falls and the pulse also, while the breathing becomes easier, and it may be that warm perspiration occurs, but this is soon followed by a reaccession of all the febrile phenomena and other acute symptoms. This change, which is in a certain sense a relapse, may not unfrequently be found to be connected with either an extension of the disease in the affected lung, or an invasion of the previously healthy one.

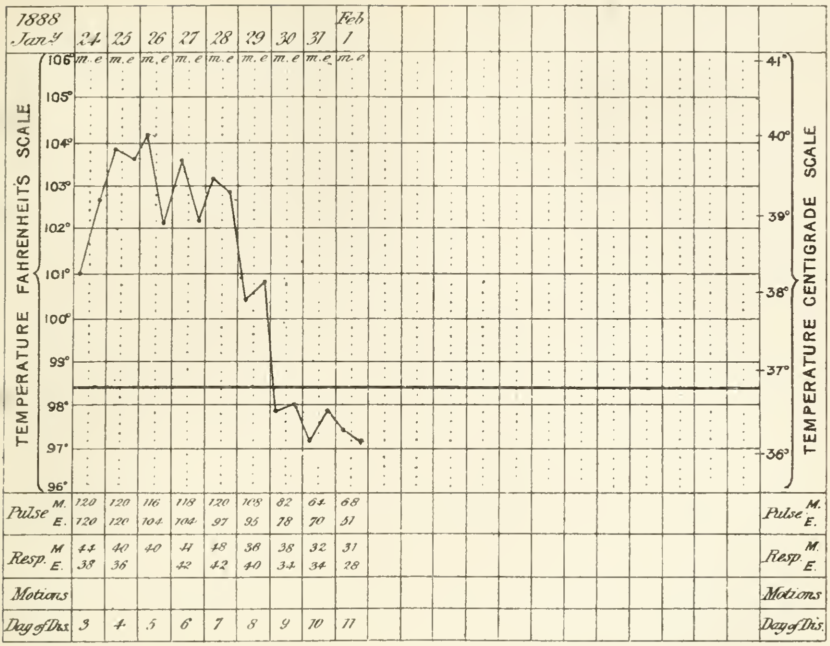
In most cases, however, there occurs a crisis of well-marked character in which the behaviour of the temperature is very noteworthy. A *perturbatio critica* sometimes precedes it, and the temperature may for a brief period attain a point higher than at any previous stage of the illness. This, however, is speedily followed by the swift fall, which may amount to a descent to normal or subnormal of a temperature which a few hours before stood at 104° or 105°.

It is extremely interesting to notice the crisis in this disease, and the manner in which it declares itself. In favourable cases the marked defervescence, and the accompanying critical events, viz., perspiration, fall of pulse, and relief to the breathing, are very striking, and all the more so that the physical examination of the lung shows as yet really no change; and they must all be present before we can say that a real crisis has come. The best form in which it can come is that represented in the two charts (see Chart, Fig. 8) taken in a case of pneumonia which has just left my ward in the Royal Infirmary, recovered. One of these charts (with morning and evening observations) shows the usual fluctuations of temperature during the height of the disease, and a sharp fall on the 7th day; the other shows the temperature in the same case taken throughout every four hours, and gives an analysis of the course of the fall during the crisis, from which it will be seen how gradually and safely for the patient the change took place, and how pulse and respirations coincided in the improvement. As was indicated already in referring to typhus, there are dangers manifold in the crisis of a pneumonia. Death may take place at the very time the critical change is progressing, and the temperature then may either sink below normal or attain rapidly a high degree. Of evil prognosis, too, is the not very unfrequent occurrence of marked defervescence at the usual time without a corresponding fall, but perhaps rather an acceleration of the pulse, for it speaks of heart exhaustion, and sudden collapse is to be apprehended. So, too, should the pulse at this time, particularly in aged people, become suddenly and abnormally slow and irregular. Treatment may, however, do much to lessen danger.

Such symptoms may no doubt occur at any period of the disease, but they are certainly more common at the crisis. Should any complication or untoward event arise during the course of the attack, it may have the effect of postponing the crisis for some days beyond the ordinary time. When no fall of temperature occurs at the expected period, but fever continues and the lung condition shows no improvement, there is reason to fear the occurrence of purulent infiltration; some destructive change in the inflamed area; or the co-existence of catarrhal

Fig. 8.

Name M.H. *Age* 32 *Disease* Pneumonia *Result* Recovered



Name M.H. *Age* 32 *Disease* Pneumonia *Result* Recovered

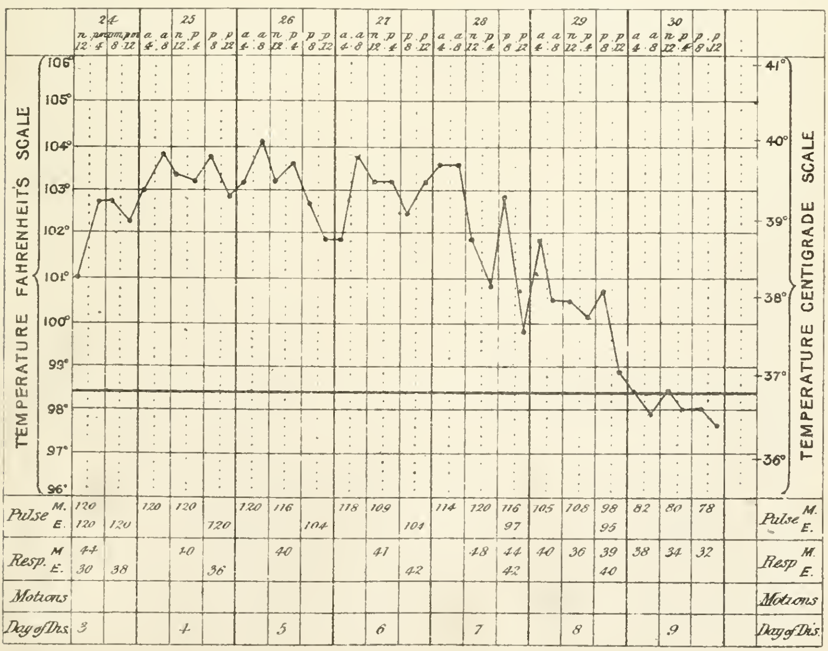
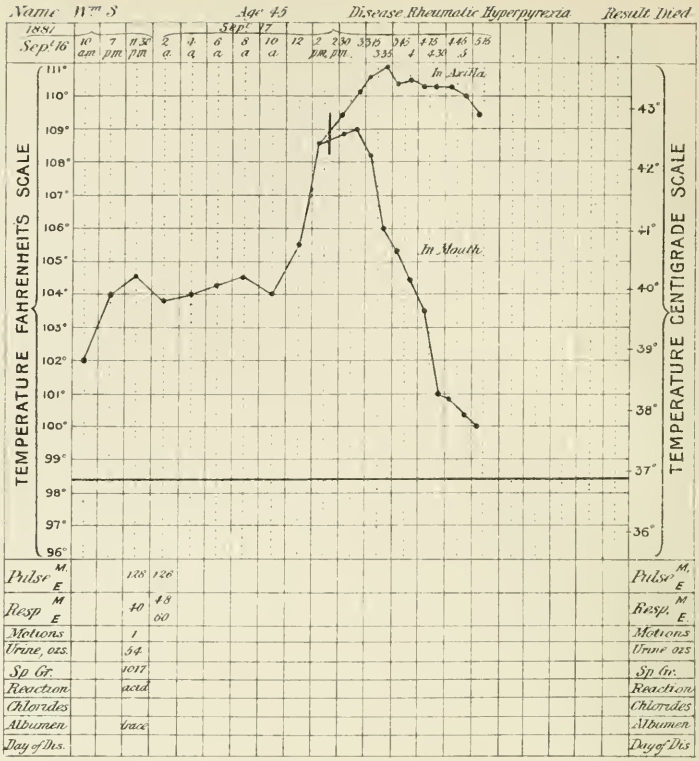


Fig. 9.


Name Wm S
Age 45
Disease Rheumatic Hyperpyrexia
Result Died
1881 Sept 16
Sept 17
In Axilla
In Mouth
TEMPERATURE FAHRENHEITS SCALE
TEMPERATURE CENTIGRADE SCALE
Pulse M. E.
Resp M E
Motions
Urine, ozs.
Sp Gr.
Reaction
Chlorides
Albumen
Day of Dis.

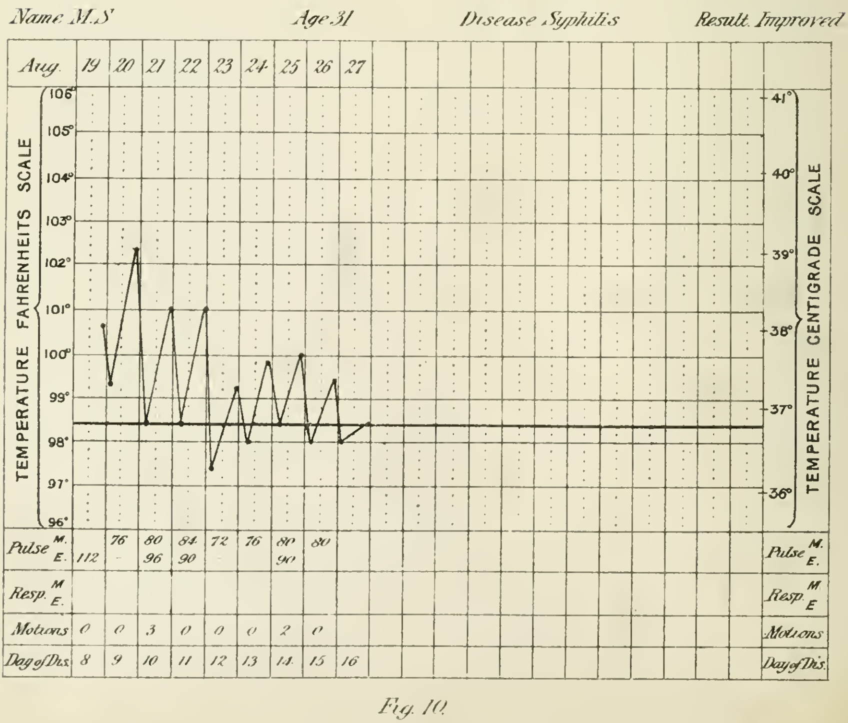

Name M.S
Age 31
Disease Syphilis
Result Improved
Aug. 19 20 21 22 23 24 25 26 27
TEMPERATURE FAHRENHEITS SCALE
TEMPERATURE CENTIGRADE SCALE
Pulse M. E.
Resp. M E.
Motions 0 0 3 0 0 0 2 0
Day of Dis. 8 9 10 11 12 13 14 15 16

Fig. 10.

pneumonia, which, as Wunderlich shows, may be allied with croupous. A pneumonia in which continuous sweating accompanies a continuous high temperature is often of rather evil prognosis, as is also a pneumonia with very marked febrile phenomena, but without expectoration. In pneumonia of the apex the temperature usually continues high for fully a longer period than in basic pneumonia, and defervescence is apt to be more in the way of lysis.

Temperature observations are of much value in *Acute Rheumatism* in reference more especially to the complications of that disease. The fever in an ordinary case is not as a rule high, and usually ranges between 101° and 103°. Its invasion is by no means so sudden as is the case in pneumonia, and its highest point is gradually attained during the first week, there being slight evening exacerbations and morning remissions. One remarkable feature of the fever is its persistence notwithstanding abundant diaphoresis. Further, while it does not appear to bear any distinct relation to the number of joints affected, and is now happily to a considerable extent under the control of remedies, it yields information of the most important kind, and quickly points out what in reality are the main dangers in this disease, namely, heart inflammation and hyperpyrexia. In any case, pursuing its ordinary course, should the temperature make a distinct move upwards, and at the same time the pulse become markedly quickened and feeble, we may suspect endocarditis even before we may be able to prove its occurrence by physical examination.

Hyperpyrexia is probably more common in this disease than in any other—a fact which suggests the careful observation of the temperature while any febrile phenomena continue during its progress. It may arise in what appears to be an ordinary and favourable case, and, while it seems to be fully more common in first attacks, also in persons who have been intemperate, and may be connected with the coexistence of acute pneumonia, it is frequently met with altogether apart from these conditions. Its onset is sometimes announced by the sudden cessation of perspiration, and by the supervention of restlessness, delirium, and other head symptoms. But the most striking thing is the rapid rise of temperature. When the hyperpyretic point is reached the whole heat regulating mechanism seems to be unhinged, and there appears to be an enormous heat production until an amount is reached incompatible with life. In the course of an hour or little more the temperature will move from 101° up to 108° or 110°, and unless the cold bath is quickly resorted to—and often in spite of it—death takes place from heart exhaustion. (See Chart, Fig. 9.) All I have observed of this complication leads me to the opinion that the occurrence of a temperature, particularly a morning temperature of 104°, gives ground for anxiously watching such a case, and directing the temperature to be taken every half hour at least; while, when it attains 105°, and appears to be still rising, the cold bath ought to be ready at hand for use in the event of the temperature reaching 106°.

There are several other forms of acute disease in which temperature observation teaches important lessons, but a mere mention of one or two of these can only now be made.

The temperature in cases of *Acute Tuberculosis*, including also acute phthisis and catarrhal pneumonia, is a valuable guide in serving to distinguish between these diseases and typhoid, to which in many points they bear no small resemblance. No doubt tubercular disease may occasionally be developed without affecting the temperature, but this must be regarded as exceptional. The type of the fever here is for the most part irregularly remittent (hectic type), and is accompanied with pseudo-crises, one or more in the twenty-four hours, which are attended with perspirations. Moreover, the breathing and pulse are usually accelerated to a greater degree than in the average case of typhoid.

Temperature readings may be of marked service in *Acute Alcoholism*, where in general it is subnormal; and any marked elevation is suggestive of some existing complication, which is not unfrequently in the form of pneumonia.

In *Syphilis*, at the onset of the secondary phenomena, there is often a considerable degree of fever; and when in charge of Fever Wards in the Edinburgh Royal Infirmary, cases of this kind came occasionally under my observation from their having been sent to hospital as probably continued fever. (See Chart, Fig. 10.)

Had time permitted I should have here referred to those extreme heights to which the temperature occasionally attains before death in many cases of disease and injury (often head affections), to which the terms pro-agonistic or pro-agonic are applied, and showed how essentially they differ from what is to be regarded as true hyperpyrexia, and how vain it is to treat them as such.

With respect to the temperature in CHRONIC DISEASES, there are many points of interest which, had time allowed, might have been presented as tending to show that while, of course, the temperature yields less precise information than is the case in acute diseases, yet thermometric observations might with advantage be more commonly made than they usually are. The subject of chronic fever present in some diseases—*e.g.*, phthisis—the tolerance of it, and its results, are topics of much clinical interest; but all that can be undertaken on the present occasion is the mere mention of some examples of chronic disease, showing temperature changes which appear to possess some significance.

In two cases of *Anæmia* of pernicious type, which had been under my care during the past two years, in which they both showed a remarkable diminution in the number of blood globules

(750,000 and 850,000 per cm. respectively), and in many other respects resembled each other,—in this also that they both recovered, one of them having been recently shown at a meeting of this Society,—there existed on admission, and for a considerable period thereafter, a notable daily elevation of temperature, which ranged from 100° to 101°·5. A permanent fall to normal of the temperature coincided with a distinct increase in the number of blood-corpuscles.

In a case of *Myxœdema*—photographs of which were lately exhibited to this Society—a prolonged and careful series of temperature observations were made at intervals of four hours, extending over upwards of three months; and in no instance did the temperature rise to the normal height, but was persistently between 97° and 98°—a point of exceeding interest in reference to the observations of Mr Victor Horsley upon animals rendered artificially myxœdematous by ablation of their thyroid.

In almost all cases of *Diabetics* under my care in hospital the temperature has shown a subnormal point.

In a case of *Infantile Spinal Paralysis* recently under treatment in my ward, which had lasted for many months, and was thus to be regarded as a chronic disease, temperature observations on the limbs, which, however well wrapped up, had always a dead, cold feeling, showed the following readings:—

Body, ...	Normal.
Thighs, ...	87°
Legs, ...	78°
Feet, ...	75°

Had the case continued under my care, I should have resorted to regular temperature observations, and any improvement in this respect might reasonably be presumed to indicate—with other things—a prospect of amendment, since the lowering of the heat in the limb is doubtless due, in no small measure, to the suspension of nerve influence.

In bringing these remarks to a close, I trust that, without claiming for this symptom of temperature in disease too much importance, some at least of the facts which have been stated may justify the view expressed at the outset of this paper, that in many maladies thermometric observations carefully made will yield the physician much trustworthy information alike in diagnosis, prognosis, and treatment.

Dr Allan Jamieson considered it was of great importance that temperature observations should be recorded by the graphic method on charts. When noted down as figures they did not exhibit the peculiar characters seen in those adorning the walls. Observations taken only twice a day showed a sudden fall of temperature when febrile processes ended by crises, but it was interesting to notice that when more frequent observations were made, the fall was seen to be very gradual. This favoured the view which Dr Affleck's experience had led him to form regarding the value of antipyretic drugs, which were largely on the increase. So long as they did not know, except to a certain extent in a gross imperfect manner, what were the causes at work in a rise of temperature, to relieve the system by such remedies appeared to him to be dangerous. This did not apply to the cold wet pack, because it in a very short space of time rose to the temperature of the patient, and became to some extent a warm application. While with kairin, thallin, etc., they certainly produced a fall of temperature very great at the time, which, it was argued, would be of advantage in relieving the patient, yet it was found that the temperature bounded up again to its former high level, and no permanent result had been proved to occur from their exhibition. Their failure, he thought, was explained by the gradual defervescence which frequent thermometric observations showed to occur. There was a curious point in regard to patients' temperatures which he had observed in hospital practice. On certain days a considerable number would show a sudden change, either a rise or an arrest of the descent then in progress. This was too universal to be attributed to anything but an influence which must be climatic in character. It was more particularly observed in typhoids and pneumonias. He had noted the temperatures of their typhoid cases against the meteorological observations, but had failed to make out any relationship. It did not appear to be due to sudden change in the wind, and he had been unable to determine whether the presence or absence of ozone had any influence. Another point of interest was the curiously high temperature which accompanied tonsillitis. There was no other affection of such a small surface which raised the temperature so high. A large diphtheritic deposit might even be accompanied by a subnormal temperature if there was no parenchymatous tonsillitis along with it.

Dr Haddon, in the course of a lengthy criticism, advocated the graphic representation of the pulse-rate and respirations as well as temperature.

Dr Clouston said temperatures bore a very important relationship to the acuter forms of mental disease. In general paralysis they could almost tell the stage and course of the disease by the thermometric observations. In acute mania the temperature was sometimes very high. In puerperal cases the use of the thermometer was of the very highest importance. It was only by it that they could tell whether septicæmia was present or not. In most forms of insanity they got no help in the detection of complications from the patient; they had to depend on their own observations, and any means of diagnosis so simple and accurate as the thermometer was just what was needed. The use of the thermometer would often prevent such mistakes as the sending into asylums cases of scarlet fever, typhoid, fracture of the skull, and other conditions apt to be accompanied by mental symptoms. He had taken thermometric observations in mental disease for the last twenty years, and had found them of the utmost value.

Dr Allan Gray claimed for the thermometer a more important place than it had hitherto held. He thought it should be much more frequently used. He had lately been having two-hourly charts taken of various acute diseases in hospital, and they showed a great number of changes that were missed when the temperature was taken only twice a day. Among other things he had observed an ague paroxysm with the morning and evening temperatures normal, but the afternoon temperature as high as 105° F. He considered that the high temperature of tonsillitis, to which Dr Jamieson referred, was explained by regarding the tonsillitis as a local exhibition of a general disorder.

Dr James Ritchie desired to emphasize the importance of frequent thermometric observations in doubtful cases. Without such care it might be easy to make errors of diagnosis in some cases of tubercular disease, and also of typhoid fever in the earlier stage when there is scarlatiniform rash and absence of diarrhœa. Temperature in health varied at different times in twenty-four hours, and the periods of highest and lowest temperature were not the same for all persons. These variations in health might modify the temperature in disease.

Dr James explained inverse temperatures on physiological grounds. The cycle of temperature was found to vary according to the diurnal or nocturnal habits of the patient. If the patient was in the habit of working by night and sleeping by day it was inverted. This had been shown by several observers, and he had made a series of observations on the night nurses in the Royal Infirmary showing that this was so. He had observed the same thing in the cases of night workers suffering from typhoid or phthisis. In connexion with this there was a practical point which was, that inasmuch as the temperature of the body showed a tendency to alter with the habit, it was well not to interfere too frequently with that. Miss Pringle had informed him that her night nurses did better with a long spell of night-work than an occasional night and day duty. In regard to the varying temperature of different parts of the body, Dr Affleck believed that it might be due to some nerve influence. It could, it seemed to him, be better explained by weakness of the circulation. Blood was constantly being heated in one part of the body and cooled in another. When the circulation was weak the transference of blood from heating to cooling parts was not so rapid as in the normal state, and thus differences in temperature would be found. He homologated what Drs Affleck and Jamieson had said in regard to antipyretics. The administration of these in large doses could not possibly be altogether favourable. The rise of temperature was in many cases salutary as evidencing a resistance on the part of the body to adverse influences within it. His practice was to give smaller doses of the antipyretics over a longer period.

Dr Smart attached very great importance to what had been said regarding antipyretics. It was a risky thing to bring down a high temperature too suddenly by such means. By doing so they were really doing violence to a natural law, which, for the time being, was a physiological compensative condition, though really pathological. They arrested the necessary changes of metabolism, which must go on, and were in danger of bringing about a crisis which was not to be desired. Referring to the case of acute rheumatism with high post-mortem temperature, he said he had four similar cases under his care while taking charge of wards during the absence of his colleagues. In all of them the temperature steadily rose, notwithstanding the antipyretic in large doses, until it reached 109° F. or thereabouts, when death, preceded by acute delirium, supervened, the post-mortem temperature rising to 110°–111° Fahr. Death took place generally within two hours after the temperature began continuously to rise. He was led to suspect the therapeutic agent which had been administered before he took charge of them. It was the salicylate of soda, and the amount given was about the same in all—from 250 to 280 grains. He believed it to be an agent of great toxic power, and had been led to introduce as a substitute the salicylate of phenol or salol, which, given in moderate and frequent doses, worked in harmony with physiological processes and reduced temperature satisfactorily. The object sought in giving antipyretics was, he believed, best attained by moderating the pyrexial condition so as to ward off hyperpyrexia; but in no case ought antipyretics to be persisted in, if temperature still rose, after they have been fairly tried.

Meeting VI.—March 21, 1888.

Dr John Smith, *President, in the Chair.*

Original Communications.

1. THREE CASES OF NEPHROTOMY, WITH REMARKS.

By A. G. Miller, F.R.C.S. Ed., Surgeon to the Royal Infirmary, and Lecturer on Surgery, School of Medicine, Edinburgh.

The following cases, whose histories I have condensed from the notes taken by the ward clerks, are given as briefly as possible:—

Case I.—J. M., æt. 18, admitted 18th March 1885; transferred from Dr Wyllie's Ward in the Medical House. Previous and family history good. Had received an injury to right loin five months previously, which was followed by hæmaturia, pain, and swelling in the right loin, frequency in making water, accompanied by pain in urethra, and pus in the urine. Bacteria found in large quantities in freshly voided urine. No history of catheterization.

26th March.—Right loin aspirated, but no pus found. Patient improved very much after this, however. Carbolized fomentations applied to loin, and sulpho-carbolate of soda administered in full doses. Dry cupping also applied to loin on three occasions by Dr Wyllie's advice. By the 25th April bacteria had disappeared from the urine, and the pus had greatly diminished in quantity. On the 30th April the patient was permitted to get up and walk about the ward.

On the *5th May* hæmorrhage occurred from the kidney, and the patient suffered much from the passage of clots, the use of the catheter affording no relief. Soon after this bacteria were noticed again in the urine, and there was an increased amount of pus.

On the *15th May*, as patient was suffering much and evidently not improving, an incision was made in the right lumbar region, and about six ounces of pus evacuated from the kidney, which was found in a considerably disorganized condition. Some of the pus was collected under the spray, and found to contain no bacteria. At the same time, bacteria were present in large numbers in the urine voided by the patient.

On the *4th June* patient is reported as doing well. There was no albumen, pus, or bacteria in the urine, only mucus. The pus discharging from the loin contained no bacteria. By the end of July the patient was considered well enough to be sent to the Convalescent Hospital, with instructions to return every week to have the dressing changed. At this time the sinus in the loin was healthy and contracting steadily. The amount of discharge was very small and perfectly sweet. No urine ever passed from the loin wound, which was dressed under the spray with sublimated wool. From this time there is a blank in the notes of the case till 4th November of the same year, when the patient returned to the Infirmary with the sinus in the loin still discharging a quantity of pus, which had become septic.

The course of the case after this is a sad one. The sinus and septic discharge continued till May 1886, when the patient died of waxy disease. During his second period in Hospital several attempts were made, by scraping, injections, etc., to stop the discharge or make it aseptic, but without success. Amyloid degeneration of the internal organs gradually developed, and the lad died of uræmia at home, fourteen months after his first admission to the Infirmary, and nineteen months after his injury. A post-mortem examination was not permitted.

Remarks on the Case.

1. There were bacteria in the urine when the lad was admitted. Where did they come from, for he distinctly stated that no instrument had been passed previous to his admission? Did infection take place per urethram?

2. The bacteria disappeared entirely under the external application of carbolic acid to the loin, and the internal administration of the sulpho-carbolate of soda, as recommended by my friend Dr Brakenridge.

3. There were no bacteria in the pus removed from the kidney by operation, while large quantities were present in the urine voided by the patient.

4. The bacteria disappeared from the urine when it ceased to be mixed with pus, the latter being all discharged by the sinus in the loin.

In regard to the treatment, I believe that had the second hæmorrhage not occurred, the lad might have recovered with the loss of the right kidney, which was thoroughly disorganized. After the operation he did well, and, had the sinus not become septic, complete cicatrization and recovery might have occurred. After the sinus did become septic, removal of what remained of the kidney might have saved his life.

Case II.—W. L., æt. 19, admitted 29th March 1885; transferred from Dr Wyllie's Ward. Previous and family history good. Seven months ago he got a severe cold and bronchitis while serving in the Royal Marines at Deal, on account of which, he says, he got his discharge from the service. Two months ago he began to have pain in his right loin, of a shooting character, and extending from the lumbar to the mammary region. Pain was increased by lying on the right side, by moving, and by pressure. Three weeks after the pain began there was frequency in making water, and a whitish deposit in the urine (pus?). No history of injury or hæmaturia at any time.

Condition on Admission.—Urine, 60 ounces; sp. gr., 1020; pus present, but no blood or bacteria. Temperature: morning, 98°·2; evening, 100° F. The right lumbar region, completely filled with a hard and painful swelling, bulging more posteriorly than anteriorly. Body drawn to that side by the abdominal muscles, which are tense and hard. On the day of admission six ounces of fœtid pus were drawn off by aspiration. Next day an incision was made in the lumbar region, and a large quantity of very offensive pus evacuated. Immediately on the evacuation of the pus the tension of the abdominal muscles relaxed, and the parietes became soft and pliable.

The after-history of this case was much the same as that of the former one in this respect, that a sinus formed in the loin, which continued to discharge septic pus till the patient's death at home in June 1887, more than two years after his admission into the Infirmary. Several attempts were made to get the sinus to close. It was frequently scraped. Counter incisions were made for drainage. A plaster of Paris jacket was worn for some time to secure rest, and the sinus was injected and packed with many substances. During the two years mentioned the patient was in the Infirmary and out of it several times, and for a considerable period enjoyed fairly good health.

Remarks.

1. In this case, unlike the former, the urine was sweet, while the pus in the loin was intensely septic. How the urine escaped contamination was explained at an operation (not the first performed) for the establishment of a counter opening, when the abscess was found to be perinephritic, completely enveloping the kidney, but not involving it.

2. The pus in the urine, which was aseptic, may have been the result of a simple inflammation of the kidney, caused by the irritation of the perinephritic abscess, and disappearing when the tension was removed. The urine became clear soon after the abscess was opened.

Case III.—M. N., æt. 22, female, unmarried. Admitted 4th August 1885, transferred from Professor Simpson's Ward.

History on Admission.—Frequency in making water, pus and bacteria in urine, swelling in left loin, liver enlarged and apparently waxy. No history of renal calculus. Swelling in loin aspirated and putrid pus obtained.

6th August.—Free incision; large quantity of putrid pus evacuated.

11th August.—Urine clear; general condition much improved; liver smaller and more natural.

Diagnosis.—Tubercular disease of kidney, which had probably existed for some considerable time, as shown by the waxy condition of the liver. Septic infection probably caused by examination of the bladder, which had been made previously to admission.

January 1886.—By this date a considerable quantity of urine was passing from a fistulous opening which remained in the loin. The exact time when urine began to appear is not recorded. Patient gradually became anæmic and hectic, and died of uræmia on the 26th March 1886. The secretion of urine, which was at one time very copious, diminished very much. She became œdematous, and was unconscious during the last two days of her life. Urine flowed through the fistula to the last.

Remarks.—Unfortunately proper notes were not taken or recorded of this case. The kidney was explored more than once for calculus unsuccessfully. A post-mortem examination was not obtained.

General Remarks.

1. *These three cases* constitute my experience of "Nephrotomy," taking that term to mean "*an incision in the loin for abscess or cyst connected with the kidney.*" The first case, that of J. M., was one of pyonephrosis following an injury. The second case, that of W. L., was one of perinephritic abscess, resulting possibly from exposure to cold, and becoming septic from contact with the colon. The third case, that of M. N., was one of tubercular disease, producing pyonephrosis. It is a curious coincidence that they all came under my care about the same time.

2. *The operation* I performed was the same in each case. An incision was made about midway between the last rib and the crest of the ilium, rather nearer the former, commencing at the outer margin of the erector spinæ, and passing outwards and forwards for about 4 inches. This line of incision was gradually and carefully deepened till pus was reached. No difficulty whatever was experienced.

3. *Results of the Operation.*—The patients were all greatly benefited for a period, the pain, inflammation, and bladder irritation disappearing almost at once. Yet in all of them a sinus formed in the loin, and persisted, from different causes, until death resulted from amyloid disease of the internal organs. The persistence of the sinus and its refusal to heal was due, in the first case, to sepsis, and the presence of the remains of a septic kidney acting as a foreign body; in the second case to imperfect drainage, on account of the peculiar shape and locality of the abscess sac, which could not contract and fill in properly; in the third case, to the tubercular condition of the kidney.

I have recorded these cases because they all terminated fatally, and I wish thus to direct attention to the fact that the operation of nephrotomy for pyonephrosis or perinephritic abscess is not so successful as some would make it out to be. The patient is immediately benefited by the operation; but, as pointed out by Mr Morris (*Surgical Diseases of the Kidney*, 1885, p. 525), "though benefited for a time, the patient may be worn out at length by suppuration or hectic."

Mr Godlee has published several cases (*Practitioner*, October and November 1887) in which ultimate recovery has taken place, though a sinus has continued to discharge for several years. These, however, were mostly persons about 30 years of age, and therefore not so liable to suffer from albuminoid disease as my patients, who were 18, 19, and 22 years respectively. On the other hand, cases have been published in the journals as *successful* who left Hospital with a sinus in the loin. If these cases were watched and reported on again, we might find them ending fatally like mine, especially if they were comparatively young. Two of my cases I might have published as successful a few months after operation.

I am unwilling to draw deductions from these cases, for the data are very few, and unfortunately post-mortem examinations were not obtained, so that confirmation of diagnosis, etc., is awanting. These cases, however, have left strong impressions on my mind, and I would like to give expression to them. Accordingly I would submit, with considerable diffidence, the following suggestions:—

1. *If a suppurating kidney is aseptic, leave it alone if you can.* The kidney may recover, or may shrivel up and become a mass of cicatricial tissue. That the latter result is possible has been frequently perceived on the post-mortem table.

2. *If incision is necessary in an aseptic case, the utmost antiseptic precautions should be employed till the wound is soundly healed;* for septic infection may mean death, especially in the young.

3. *If the suppuration is septic, prefer nephrectomy to nephrotomy, provided the other kidney is sound.* Incision may be employed advantageously as a preliminary to excision. Such a procedure, according to Otis (*Boston Med. and Surg. Journal*, October 1887), "robs a subsequent nephrectomy of much of its danger" (the mortality of the latter varying, according to his tables, from 31·48 per cent. to 54·44 per cent.)

Mr Godlee (*loc. cit.*) also says that a contracted kidney is removed with less risk to life than a large one. "Hence the great advantage of draining such a kidney, and allowing it to shrink before taking it away."

At the conference held in Paris in 1886 (*Revue de Chirurgie*, November 1886), the general opinion seemed to be in favour of nephrectomy, preceded by nephrotomy, being the best treatment for suppuration connected with the kidney.

Mr Cathcart thought Mr Miller, in speaking of "nephrotomy," had not sufficiently drawn attention to the different kinds of disease for which the operation might be required. If the diseases were of a different character it seemed hardly right to lay down lines of treatment that might be suitable for them all. The particular treatment of a suppurating joint or organ, say the testis, would vary with the character of the disease. Incision in simple cases, probably excision or scraping out in tubercular. It would be generally accepted that the same doctrine was true for the kidney, the difference being that the one organ was not so easily removed as the other. He did not quite follow Mr Miller in his view, that the kidney in one of the cases should have been removed because of a septic abscess round it, unless one could say that the kidney itself was necrotic, and he did not gather that such was the case.

Mr Miller said he brought forward these cases mainly because they had been fatal, and to point out that the operation was not so harmless as was generally supposed. His deductions were from these only, and he did not wish to lay down general principles for the performance of the operation.

2. *Dr Smart* described SOME FORMS OF UNDESCRIBED RESPIRATORY NEUROSES.

Dr G. A. Gibson gave expression to the pleasure with which he had listened to Dr Smart's communication, and to his regret that he had not been in time to hear the whole paper. He was of opinion that a simpler explanation might have been proposed for the phenomena represented in the third tracing shown by Dr Smart, and thought that exhaustion of the central nervous mechanism controlling the respiration would satisfactorily account for it. The tracing in question was characterized by a regular periodicity. Now, periodic respiration has been described as occurring in the frog under certain circumstances, in the dormouse when hybernating, in the dog when worn-out by prolonged exertion, and in man during the profound slumber which follows severe labour. The tracing to which he called attention had some resemblance to one given by Mosso in a recent work as the graphic representation of periodic variations in the respiration of a healthy but exhausted dog. Considerable discussion had been caused by the different attempts made to explain periodic respiration, and he was led to make a few remarks on the subject by Dr Smart's reference to the implication of the vaso-motor system in the symptoms which he described. Periodic breathing was seen in its most pronounced form in Cheyne-Stokes breathing. Since the classical explanation advanced by Traube and challenged by Filehne, the subject had received much attention, and many ingenious attempts had been made to solve the question of its causation by such authors as Biot, Franck, Cuffer, Grasset, Rosenbach, Fano, Murri, Luciani, and Mosso. Some of these writers regarded Cheyne-Stokes respiration as being entirely distinct from other arrests of breathing; others held it to be merely the most fully developed example of a series of phenomena linked by gradations. He would like to call attention to the fact that along with the alterations in the breathing there were frequently changes in the rate and tension of the pulse, in the size and reaction of the pupils, in the condition of the muscles, and in the state of the mental faculties—all these phenomena accompanying the periodic variations of the breathing. Every theory which had been advanced postulated a condition of lowered activity of the respiratory centre, but here the likeness between the explanations ended, for some of them regarded the depressed state of the centre as due to changes in the quantity of the gases contained in the blood, others as being caused by simple exhaustion of the nerve-cells. Of the theories referred to those of Rosenbach and Filehne might be regarded as representative. The former attributed the complex of symptoms (respiratory, circulatory, visual, muscular, and mental) to exhaustion of the whole nervous centres, showing itself in exaggeration and variation of the normal periods of rest found in health—the latter demanded not only that the activity of the respiratory centre should be reduced, but that it should be depressed to a lower level of excitability than the vaso-motor centre, and by this theory the arterial spasm caused by the stimulation of the vaso-motor centre interferes with the access of oxygenated blood to the respiratory centre, and thus insures and prolongs the excitation. He could not accept the theory of Filehne, as the phenomena brought forward in support of it were not constant, and he was in the meantime inclined to support the larger and more scientific explanation of Rosenbach.

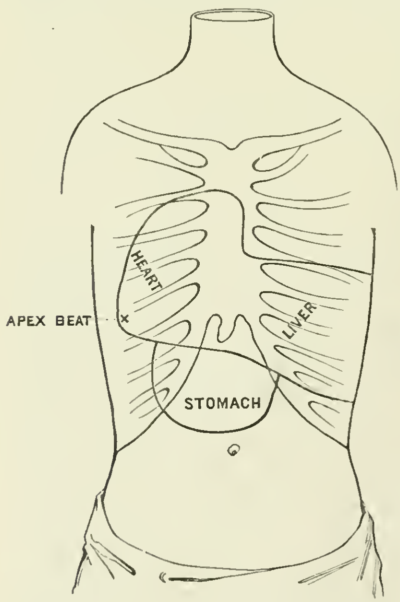
Dr Smart replied that had Dr Gibson been present during the earlier part of his communication he would have known that his (Dr Smart's) cases belonged to a special, rare, and undescribed category, which could not be explained by "exhaustion of nerve cells," as can many kinds of abnormal respiration of more frequent occurrence. He, besides, objected in any case to the phrase "exhaustion of nerve cells," as too vague. The "exhaustion" referred to is necessarily induced by antecedent changes, which have to be accounted for and explained on sound physiological grounds.

Meeting VII.—April 4, 1888.

Dr John Smith, *President, in the Chair.*

I. Exhibition of Patient.

Professor Thomas R. Fraser showed a case of TRANSPOSITION OF THE THORACIC AND ABDOMINAL VISCERA in a married woman, 23 years of age. She had given birth to her first child in December 1886, and while nursing the child had suffered much from weakness, and had fainted several times. This illness seemed to have been partly caused by excessive and prolonged lactation, but it was also an accentuation of an experience of giddiness, faintings, and breathlessness from which she had occasionally suffered since childhood. It is probable that the displacements of the viscera would have escaped attention but for the circumstance that a few months ago she accidentally discovered a beating when her hand was placed near the right nipple, which greatly alarmed her, as she imagined it was caused by a "beast in her breast trying to make its way out." The discovery made her very nervous; and, probably because her attention had been drawn to its occurrence, the sensation of beating became a frequent and distressing one, and it soon became associated with severe attacks of dyspnœa. She afterwards had a gastro-intestinal disorder, which confined her to bed for several weeks. She was attended in this illness by Dr Johnston of Leith; and during it the palpitations, dyspnœa, and giddiness became still more distressing. As the latter symptoms continued after her recovery from the gastro-intestinal disorder, she was sent for examination and treatment to Dr Fraser, who expressed his great indebtedness to Dr Johnston for the opportunity of determining the remarkable abnormalities that were present. Dr Fraser saw her at the Royal Infirmary on the 2nd of March. She then complained of breathlessness and palpitation, and indicated the right mammary region as the situation where the palpitation was felt. On placing the hand on this region, a well-marked impact was felt, with a distinct presystolic thrill; and on auscultating, the heart sounds were heard to be accompanied with a distinctly audible presystolic murmur. On proceeding to percuss the chest, it was found that there was dulness over the greater part of the right side, and a clear resonant note on the left side from the supra-clavicular region to the fourth rib, where also the respiratory sounds were clear and of normal character. It was obvious that the heart was displaced. Inquiry showed that there had been no illness to account for the displacement, for with the exceptions stated the patient had enjoyed singularly



good health throughout her life. The other non-symmetrical viscera were then searched for, and it was found that the liver was also displaced, the greater part of it being situated in the left side; the right lobe, or what corresponded to the normal left lobe, extending across the epigastrium to within a short distance from the apex beat of the heart on the right side. It was also found that the stomach was transposed, the greater portion of it being situated on the right of the middle line, and that the spleen was on the right side in place of the left side, its dulness being found between the eighth and tenth ribs in the right axillary region. A diagram of the anterior aspect of the thorax and abdomen was exhibited to illustrate the position of these viscera. It was pointed out that the viscera are placed in higher positions than is usual, a circumstance accounted for by the patient being in the fifth month of a second pregnancy. The condition of the patient prevented an examination from being made of any other important structures in the abdomen or pelvis, but an opportunity would probably be afforded for this being done after delivery. Inquiries and observations were made which showed that the patient is righthanded and not lefthanded when sewing, writing, feeding, etc., and this was confirmed by the results of testing the power of each hand in grasping the dynamometer, the right hand being considerably more powerful than the left. The question of heredity had also been considered in connexion with this remarkable freak of development. The patient's grandparents are dead, but no abnormality was known to have existed in them, and they had been singularly healthy and had attained to an old age. The father and mother are alive, and had been examined by Professor Fraser's resident physician, Dr Tofft, and their viscera had been found to occupy the ordinary situations. The patient herself is the seventh member of a family of eleven, of whom two, a brother and a sister, are dead. Of the survivors, six sisters and one brother had been examined (several of them, in kind compliance with Dr Fraser's request, by Dr Johnston), and had been found to be normal. The patient's own child, fifteen months old, had likewise been examined, and the viscera were found in their usual situations. Although no hereditary tendency to transposition of viscera had been discovered, a tendency to cardiac disease appeared to exist in the family, as the mother and three sisters suffer from cardiac disorder, and in three of them the lesion is, as in the patient, a presystolic mitral one. The patient had been under treatment in the Royal Infirmary for the cardiac symptoms produced by this lesion, and the breathlessness and palpitation have now disappeared. This result had a special interest to Professor Fraser, as he has thereby had the opportunity of relieving, by the administration of strophanthus, the symptoms produced by disease of a heart occupying so unusual a situation. It was pointed out that the case agrees with the great majority of those that have been recorded, in which transposition of the heart is accompanied with transposition of all or nearly all of the other non-symmetrical viscera. In the absence of organic disease, the transposition does not appear to cause any inconvenience, and it is compatible with a prolonged and healthy existence. The Professor added that of 78 cases recorded, in 70 all the viscera were transposed, and in 8 the heart only. In only a few of the recorded cases had the transposition been discovered during life.

II. Original Communications.

1. A CASE OF CEREBRAL INJURY FROM A FALL.

By William W. Ireland, M.D.

On October 14th, 1885, about half-past five P.M., a party of four persons were driving to Hawick in a four-wheeled dogcart. The horse was frightened by some children, one of whom was waving a flag. The animal ran away in uncontrollable fright, and dashed the vehicle against a lamp-post. The dogcart was wrecked, and the horse fell dead, though no marks of injury could be found on its body. None of the three young people thus rudely thrown out from the dogcart were seriously injured, but Miss J. M. was found lying about twenty paces behind the spot where the course of the vehicle had been arrested. She must either have been shaken out by the violent motion, or have fainted and fallen out. She was immediately carried into an adjacent house by my son, who had been thrown out from the vehicle but not much hurt, and two medical men speedily arrived. They found her in a state of collapse, almost pulseless, blood issuing from the mouth and nostrils, and also some trickling from the left ear.

My son telegraphed for me, and I reached Hawick about 11 P.M. that night. The patient was lying on a sofa quite insensible, with irregular weak pulse and stertorous breathing, which continued the whole night. Vomiting of blood took place several times during the night, no doubt from blood which she had swallowed. Next morning, after consultation with Dr John R. Hamilton, she was borne in the ambulance waggon to the Hawick Hospital, care being taken to avoid shaking as much as possible.

On reaching the Hospital about 9.30 her condition was carefully observed. She had been thrown on the macadamized road from a vehicle going at a rapid rate. Apparently she had alighted on the left side of the head. There were marks of a bruise on the left side above the left ear, and some injury to the zygoma in front of the ear. There was oozing of blood from the left ear. There was ecchymosis and much swelling of both eyelids. This swelling of the eyelids, as well as external strabismus of the left eye, was noticed when she was first picked up. The right arm and leg were completely paralyzed. There was bruising of the right arm and right hip; a spot of suffused blood at the latter place measured 16 inches by 9 inches. A private room was taken for her, and an experienced nurse procured. Dr Hamilton assumed charge of the case, while I engaged to come now and then. I went to Hawick five times, generally on the Saturday, returning home on the Monday. The treatment adopted was absolute rest and antiphlogistic diet. The hair was cut and ice applied to the head. She was quite insensible. The prognosis was serious, especially as she was 48 years of age, but her friends were told that recovery was possible.

16*th October*.—Slightly moved right arm and leg. Blood ceased to come from the ear. Left pupil slightly reacted to light; the right always did so.

It seemed to me that the condition of the soft parts within the cavity of the skull was probably the same as seen in the eyelids. Owing to the great violence with which she had been dashed to the ground, there had been a rupture of many of the smaller vessels, and an effusion of blood under the arachnoid of both hemispheres of the brain, especially on the left side. There was a slight protrusion of left eye, probably owing to effusion behind in the cavity of the orbit.

19th October.—Some oozing of blood observed from left ear.

22nd October.—The head was shaved and blistered over the left parietal bone. After this we went on blistering the head from the left to the right side at intervals of three or four days.

25th October.—On introducing the end of a clinical thermometer into the meatus of the left ear to take the temperature, there was a slight oozing of decomposed blood.

2nd November.—In the evening she spoke and asked where she was. The suffused blood in the bruises on the body disappeared about this time. J. M. had thus been nineteen days unconscious of her condition.

5th November.—Great general improvement, and return of motion and sensibility in the right side.

7th November.—I collected or took the following observations.

The temperature in the axilla had been 99° in the morning and 100° in the evening for the first ten days, after which it had been 98·4° in the morning and 99° in the evening. The pulse was variable, ranging from 90 to 130. On several occasions it was observed to be weaker in the right arm, which was colder at the time. Once or twice the pulse was scarcely perceptible on the right side, while bounding on the left. On that day the temperature at the axilla was 98·4°; the pulse 80, soft, and readily quickened.

The head temperatures were on the right side behind the ear 97·4°, on the left side 97·8°. The temperature in the meatus of the right ear was 97·1°, in the left ear 97·8°. The protrusion of the eyeballs had subsided, and there was no discharge from the ear. The smile was unequal from paresis of the left side of the face. The swelling of the eyelids had disappeared; she could not open the left eye readily. Could look up and down, but outward motion of the left eye was impaired. There was no paralysis of the right rectus internus. Saw pictures on the wall before her double. There was a noticeable thickness in her speech, and a slight difficulty in swallowing, or, as she defined it, a feeling of tightness in the throat. No impairment of the muscles of the jaw, uvula, or pharynx could be observed. The intelligence was childish; slept well. Bowels costive.

12th November.—Able to sit up for half an hour. *13th.*—Complained of pain in the left eye.

17th November.—Great intolerance of light and irritability in left eye. Blister applied on left temple.

22nd November.—Face unequal in smiling, less so at upper portion of face than below. Difficulty in completely opening left eye. Paralysis of external rectus, and slight paralysis of inferior oblique. She said that she saw near and far objects double. On putting out the tongue it deviated a little to right side. Voice still thick. Stiffness in swallowing has been away for ten days. Intelligence now returning, memory stronger. Nerves affected, third, sixth, seventh (facial), and perhaps also ninth. The site of lesion might be the posterior portion of pons and floor of fourth ventricle, where these nerves take their origin. Paralysis of right arm and leg might be due to lesion of left side of pons.

On the 25th November, 42 days after the injury, she was removed to her home in a saloon carriage, a railway journey of about 50 miles. She was able to walk from the cab into her house, but from the weakness of her pulse and other signs she seemed near the end of her strength. As generally happens in such cases, the exertion was followed by depression, most felt the day after.

It was noted on the day she reached home that the facial paralysis had almost disappeared, and the eyelid was recovering power; but the half-shut condition of the eye was still noticeable. The paralysis of the external rectus was not yet quite gone.

For months she remained weak, walking with a feeble and tottering gait, but continued to get stronger in mind and body.

After the lapse of two years and a half from the date of the accident, it may be fairly said that she has made a good recovery. The only traces of the paralysis is a slight drooping of the left eyelid, which is worse when she is fatigued.

The question occurs—Was there a fracture of the base of the skull? It certainly is not possible to prove that there was none, yet it seems to me that there were no decisive signs of such a fissure, and that all the cerebral and nervous symptoms could be accounted for by the supposition that they were owing to bruising of the cerebral tissues and pressure from effused blood. That is to say, owing to the violence of the shock there was a rupture of the smaller vessels of the brain, and orbits, eyelids, and mucous membranes of the nasal passages and ear. I thought at the time that the absorption of the effused blood in the bruises on the body would be an index of what was going on within the cranium, and this was fulfilled by the return of consciousness about the same time as these superficial effusions disappeared. The pressure upon the brain was relieved by the absorption of the blood effused within the cranium. Those surgeons who may hold that the effusion of blood causing tumefaction of the eyelids and bleeding from the nasal passages and from the ear, as well as the paralyses of the several cranial nerves, were all owing to fracture of the base, ought to be able to indicate on a skull where the fracture ran. It must have been a pretty extensive one, implicating the temporal, sphenoid, and ethmoid bones, and probably also the basilar process of the occipital. As regards the discharge of blood from the left ear, it was neither great nor continuous, and there was no discharge of serous fluid. It is worthy of notice that there seemed no paralysis of the soft palate and uvula, which is described in some text-books of surgery as following divisions of the facial nerve from fracture of the temporal bone, before the great petrosal branch is given off, or before the division of this branch itself ere it joins the spheno-palatine ganglion. At least as far as my experience goes paralysis even of one side of the soft palate and uvula is attended with inability to pronounce the gutturals *g* and *k*, which did not occur in this case.

It has been observed that after recovery from periods of unconsciousness following similar injuries to the brain, the patient has forgotten occurrences which took place before the accident, especially those which immediately preceded the injury.

It was therefore interesting to note what memory was retained in this patient of the passages in her life before this unfortunate event. It was ascertained by careful inquiry that she remembered the horse running away, but not her falling out. Before that day she had spent a week with her sister at her house on the hills, but this had entirely passed from her memory, and indeed she calmly asserted that she had never been there. On being asked what she was doing in the dogcart so far away from her home, she said she was going to the house, not coming from it; whereas she was driving to the railway station to return home at the end of her week's visit. All events—meeting with friends and conversations—during this week were blotted from her memory, nor could they ever be subsequently recalled. Even before this time the memory of some events was wanting. On the 28th September a relation and his wife had come to see her. He stayed a few hours, and then went away, leaving his wife, who remained with J. M. for a week. She remembered him coming, and that he came from Moffat, but insisted that his wife was not with him. The two ladies had gone together to make some inquiries about a boy's school. On being reminded of this, J. M. said that she remembered going to the school, but that she went alone. For months after the accident her memory was noticed to be weak and treacherous. This obliteration of memory after an injury to the brain has been the subject of a very able paper by Dr Joseph Bell, which is published in your *Transactions* and in the *Edinburgh Medical Journal* for February 1883.

In a paper upon this question of "Loss of Consciousness,"[1] Dr Frank Hamilton, President of the Medico-Legal Society of New York, inquires whether the period of oblivion bears any relation to the nature and gravity of the cerebral injury. He has collected twenty-six cases in which the loss of memory could be dated back to a definite period. In several accidents from vehicles the patient

[1] *Papers read before the Medico-Legal Society of New York.* Third series, 1886, p. 206.

seemed to have been able to recall some particular circumstance which impressed the mind immediately previous to the injury. Thus one man remembered feeling annoyed by the obstruction of another carriage which was the cause of his own being upset, but nothing more. Another man, thrown from a buggy, insensible for ten minutes, remembered only that his horse was running away, and that he leaned forward in the buggy. In many cases the patient remembered the impending danger, but not the injury.

Mr A. G. Miller said that an interesting point in the case was whether there was or was not a fracture. An examination of the membrana tympani would have aided the diagnosis. When bleeding from the ear occurred in these head injuries, there was usually either fracture of the base of the skull, or else fracture of the osseous meatus from the condyle of the jaw being driven forcibly against it. In a case in which the blow came on the symphysis of the jaw, there was bleeding from the ear, and the patient was for a time unconscious. He was thought to have sustained a fracture of the base, but got well too quickly for that. On examination there was found not a rupture of the membrane, but a fracture of the osseous wall of the meatus. An American police surgeon had recently published some observations that tended to throw light on some of these obscure injuries. He found from a series of post-mortem examinations, that where the internal injury was in the neighbourhood and on the same side as the external injury there was fracture, but that if the extravasation was not the result of fracture, it was on the opposite side of the head—contre-coup. Another point he observed was that where there was very little external injury there was more likely to be damage to the brain than where there was a scalp wound. Mr Miller added that he believed there was a fracture in Dr Ireland's case, from the subconjunctival ecchymosis and the position of the cerebral injury on the same side as the blow.

Mr Cathcart thought that the vomiting of blood in the case, taken along with the symptoms on which Mr Miller had dwelt, indicated the presence of a fracture passing also across the roof of the nose and pharynx. The blood poured out would pass down the pharynx and be swallowed, and the vomiting of the blood was thus easily accounted for; otherwise it was difficult to understand. He thought from the description of the accident that there was fracture of the skull as well as laceration of the meningeal vessels, as Dr Ireland had supposed.

Dr Clouston said such cases as Dr Ireland's had a very high psychological interest. Hitherto they had heard much of the surgical and medico-legal side of such cases, and very little about the mental functions of the brain which were affected. In such a case as Dr Ireland's, they had a series of symptoms of the greatest possible interest, and he was very glad that it had fallen into the hands of a man like him to observe the mental as well as the bodily symptoms. They of the mental branch of the profession had real cause for complaint against their surgical brethren, because of the way in which they neglected the mental symptoms. He had seen many cases of this kind recorded in which it never seemed to have occurred to the writer that there was any mental function of the brain at all. Traumatic injuries of the brain brought out mental symptoms in two ways. If there was heredity to mental disease, they brought out in some cases an attack of what might be called ordinary insanity. The cases of the second sort, where they had had special mental symptoms directly attributable to the injury of the cortical texture of the brain, were of especial interest. For example, in nearly every instance of such cases they had two resulting symptoms. The one was the loss of the higher self-control over speech, thought, and action, the outward manifestation of which was the irritability to which Dr Ireland had referred, and also intolerance towards certain drugs as well as alcohol. These cases lost the power of controlling the effect of alcohol, which in the smallest quantity caused either delirium, intense headache, or premature drunkenness with violence and impulsiveness of an unrestrained character. There was no better proof of the delicacy of the human brain, and how it required to be sound than to watch the effect of a blow, which had ruptured many small vessels, on the mental nature of a man or woman afterwards. Such people could seldom go through sustained exertion. They could not go on with their work, nor bear subjection to worry as before. Many were subject to intense cephalalgia of a very terrible character. The moral faculties in most were altered. He thought they needed a very great deal more observation in regard to the mental or cortical effects of brain injury, and he had no doubt that if these mental symptoms were more acutely observed, if the terminology in which ordinary mental symptoms could be expressed were more used and better known, they should have more knowledge of the brain itself. In regard to the diagnosis, he would take issue with Dr Ireland as to there being rupture of the pons, or an injury on the floor of the fourth ventricle. Rupture of a vessel in the floor of the ventricle was so deadly that they could hardly agree that such occurred. He would rather believe that rupture of vessels or a pressure from clot had occurred around the nerves as they issued from the brain. He had no doubt that in such a case there was an immense number of capillaries burst. It had been shown by Duret, that a very slight blow to the head of an animal was sufficient to cause rupture of a number of capillary vessels, and they were caused in a great many directions. They scarcely realized that the brain was a moving organ floating in fluid, and that therefore they were apt to have a number of such bursts from injuries. He had seen an immense number of capillary apoplexies in various parts of the brain, and especially in the brain cortex, in post-mortems after injuries. The symptoms mentioned by Dr Ireland in regard to memory were extremely interesting. They seemed to show that the registered impression through the senses on the cortex of the brain was not an impression that was written very clearly, or in a vivid way to begin with. It was very like a dream. Many of them dreamed constantly. If they did not talk of these dreams and bring them up to consciousness they forgot them very readily. But if they did talk of them, they became so written in on our memories that they did not forget them readily. It seemed, further, as though some of the impressions written in vividly enough at the time, yet because of their recent occurrence before such injuries, had not become sufficiently "organized" in the brain cortex to be remembered. Before concluding, he would mention a surgical point in connexion with Mr Miller's remarks. One patient of his struck another with the flat side of a shovel. There was no external injury that could be made out by four physicians even after the scalp was shaved, but at the post-mortem they were astonished to find a fracture of the base of the skull with marked laceration, and apoplexy of the cerebral tissue.

Dr Ireland, in reply, said that with reference to the fracture of the skull he did not consider any one symptom as pathognomonic. It was necessary to analyze all before coming to a conclusion. He could not recollect whether the membrana tympani was examined or not. He had not done so himself. There was, however, no interference with the hearing power. As to the subconjunctival ecchymosis, it was present in both eyes. If due to fracture, both wings of the sphenoid must have been involved. The vomiting of blood, he thought, was due to blood passing from the naso-pharynx into the stomach. He agreed with Dr Clouston, that surgeons might give great assistance if they recorded the mental as well as the surgical symptoms in such cases.

2. TROPICAL MALARIA AND ITS SEQUELÆ.

By George Dods, M.D., L.R.C.S. Ed.

I have ventured to bring to the notice of the Society this evening the subject of Tropical Malaria and its effects, as seen in the Anglo-Indian after his return to Europe. From whatever disease he has been invalided, that disease is pretty sure to be influenced, to a greater or less extent, by the malarial taint. Most medical practitioners in this country have now and again a returned Anglo-Indian among their patients, and I thought that the subject of malaria (with which circumstances have made me somewhat familiar) might be of some interest to the members of this Society. The intercourse with our tropical and subtropical possessions has increased of late years to such an extent, that the tide of returning invalids is not likely to diminish; and although from various causes the death-rate has decreased to about one-half of what it used to be, still the diseases usually styled tropical—such as fever, dysentery, hepatitis, etc.—are many times more fatal than at home.

Living in the tropics, the European is under the influence of two causes which are not in operation in more temperate climates, namely, continued high temperature and malaria. As long as the individual lives within the tropics these two agencies are at work, lowering his vital powers and producing that asthenic condition which inevitably overtakes every European, or at least every Anglo-Saxon who lives there. He never gets acclimatized, and is as liable to sunstroke after a residence of a dozen years as he was during the first week of his arrival. Almost every one who has resided for a lengthened period in the East has imbibed the malarial poison, although he may never have had an attack of fever. In some its effects are scarcely manifest, while in others derangement of the internal organs, perhaps splenic enlargement, anæmia, and various other lesions betray its presence. But from whatever disease they may suffer, the symptoms will be modified, and the treatment interfered with, by the presence of the malarial taint; it will mask the symptoms and obscure the diagnosis, and therefore in the case of Anglo-Indians the probability of its presence should never be lost sight of. It is not the attacks of disease of a severe type which so much concerns the European in the tropics, as the slow and constant effect of living in an elevated temperature, and being nearly always exposed to the malarial influence.

This peculiar entity which we call malaria, the presence of which is known by certain phenomena induced in the human subject, but whose actual existence cannot be demonstrated, attains its greatest intensity in the tropics. It has the power of drifting along from its source for a considerable distance, and to be able to roll up the sides of mountains and along ravines, and yet can be stopped by a high hedge, a grove of trees, or even by a musquito curtain. It prefers low levels, but is found also on the tops of mountains. It coexists with vegetation, and is found in arid wastes, utterly devoid of any vegetable growth. It is lessened where the ground is cultivated and there is an adequate population, and is increased where the vegetation is destroyed and the soil exposed to the sun. It seems to cling to the skirts of forests and along the forest paths, as though it had drifted off the neighbouring plains. It is scarcely met with in the depths of the forest, but advances rapidly on the track of the pioneer, who begins to clear the ground.

Dr Oldham, of the Indian Medical Service, does not believe in the existence of malaria, but I do not think that he has many followers. He ascribes all the phenomena to a chill; but a chill is more the *post hoc* than the *propter hoc*, seeing that most fevers begin with a chill. A chill or exposure to sudden cold would in all probability produce an attack of fever in a person subject to the malarial influence or who had imbibed the taint; but no amount of chill would bring on an attack of intermittent fever in a non-malarious locality, such as the Arctic regions, or out upon the ocean.

Klebs, Tommasi-Crudelli, and some others, maintain that malarious fever is caused by the presence of a minute bacillus, which is found in the air and water of certain localities, and is identical with one found in the blood of those suffering from intermittent fever. It would be very satisfactory proof of the existence of this "bacillus malariæ," as a cause of periodic fevers, could it be demonstrated on the human subject, and on some one who had never been exposed to the influence of the poison, as the comparatively harmless results likely to follow an inoculation need be no obstacle to the experiment.

I believe an attack of ague was said to have followed the inoculation in the case of a dog which was experimented upon, but I feel rather sceptical about the diagnosis of ague in a dog, seeing with what remarkable facility that animal can get up a shivering fit. In the years that have elapsed since the discovery of this bacillus, we have had very slight corroborating testimony from independent observers. In the vast malaria-stricken districts of India and Assam, there must be numbers of interested investigators, and ample material to work upon, and yet we have heard very little of the bacillus malariæ. It does not seem to have yet been proved that this particular bacillus is pathognomonic of paroxysmal fever. Even if it were proved to be so, I do not see that it would make much difference in either prophylaxis or treatment. Whatever it is that causes these fevers, we certainly recognise the presence of something more than the result of mere climatic influence, something more than is produced by the various conditions of climate and locality. Some discredit is thrown upon the very existence of malaria by the term *marsh* miasmata, as the paroxysmal fevers occur in situations the very opposite of marshy, and the necessity for finding a paludal source for several unaccountable outbreaks of fever amongst our troops on foreign stations was the cause of some curious statements in the Army Medical Reports of former years. The island of Hong Kong has long enjoyed an unenviable notoriety for its fevers, which certainly cannot be said to be the product of marshy ground, as there is no marsh and scarcely any level ground on the island. When we first landed the troops there their health was excellent; but afterwards, when the island was ceded to us, and we began to break up the ground for roads and buildings, a remittent fever of the most malignant character at once broke out. In the first year of occupation 10 per cent. of the civilians died, and the 59th Regiment lost 24 per cent. of its strength. In 1865, when some extensive levelling and road-making was in operation near the camp, the troops again suffered severely from fever. In 1870 a certain cliff, composed of disintegrated granite, was scarped and cut away to furnish material for the repair of the roads. Every house within 500 yards of the cliff had one or more cases of fever, many of them proving fatal. Some ground was levelled near the summit of the mountain, at a height of 1000 or 1500 feet above the sea, on which some bungalows were built, and the same kind of fever broke out when the houses were occupied. I have mentioned these facts to show that malarial fever of the most deadly nature may abound in localities far removed from marsh or jungle. The malaria arising from newly excavated ground seems generally to give rise to fever of the remittent type—the type which is the most deadly. The fact of the most fatal form of fever prevailing in localities the very opposite of marshy, and invariably following any disturbance of the soil, is a fact of the greatest importance, and ought to be fully recognised. And yet in our tropical and subtropical possessions and military stations, it is quite the rule that ground should be levelled and excavated, and then a camp formed, without the slightest regard to the outbreak of fever, which is almost certain to follow. When our troops went to Cyprus in 1878, the men were quartered on ground which had been recently disturbed for road-making and levelling. In the six months following, there were 2203 cases of remittent fever. So late as 1885, a detachment of artillery were sent to garrison a newly constructed fort in Hong Kong, with the result that so many cases of remittent fever occurred, the men had all to be withdrawn. Although the fever seems to declare itself as soon as the soil is disturbed, yet whenever the vegetation again covers the ground the fever disappears. In the whole of 1880 there were only seven cases of remittent fever in Cyprus, where two years before there had been over 2000 in six months.

The intermittent variety of fever abounds on the deltas of rivers, in the vicinity of partially flooded land, on tidal mud banks, marshes, and rice-fields. So long as the rice-fields are covered with water or the crop is still growing, the malaria is in abeyance; but as soon as the crop is harvested, the land drained of water, and the ground exposed to the sun, fevers begin to appear. So entirely different are the conditions of soil and climate under which the intermittent and remittent fevers are met with, that I am inclined to believe there are different kinds of malarial poison, causing two different kinds of fever, one remittent and the other intermittent. Sir Joseph Fayrer says, "The malaria of an arid district may act differently from that of the jungle or swamp; and considering the different climatic conditions under which it originates and operates, it is not strange if it is so."

Seeing the very varied conditions under which malaria appears or is developed, it is almost impossible to point to any combination of circumstances which will certainly produce it. The first essential seems to be a hot sun or high temperature and a damp soil. It does not seem necessary that there should be decayed vegetation near, or, indeed, even organic matter in the soil, although this is usually considered essential; for we find the soil of Hong Kong, which seems to be so fruitful of malaria, has not above 2 per cent. of organic matter in its composition. And so far from luxuriant vegetation being a cause of malaria, there is abundant evidence to show that malaria springs into existence when a country is deforested, and that many districts possessed of little or no vegetation, formerly uninhabitable from malaria, become healthy as soon as trees begin to grow. The question, "What *is* malaria?" yet remains unanswered. All that we know is, that in the tropics a hot sun acting on a moist soil sets free some agent which causes paroxysmal fevers.

When a European goes to live in a hot climate, the usual result of an elevated temperature, especially if combined with a moist atmosphere, is a relaxing and enervating of his whole system, impoverishment of the blood, and anæmia, more or less intense. During a hot summer in these latitudes, the same effect may be observed in a modified degree; but in the tropics, where hot weather is the rule and not the exception, there can be no doubt about the injurious effects of continued heat on the blood of the European. There is less necessity for animal heat, hence less demand for food, less metamorphosis of tissue, and decreased excretion. There is a diminution in the number of red corpuscles in the blood and in the quantity of oxygen inhaled. On account of the heat there is very little exercise taken, and if taken at all, it is in very moderate quantity, as fatigue is so easily induced. The languid respiration not being stimulated by healthy exercise and accelerated by movement, there is considerably less oxygen inspired, and the decarbonization of the blood is largely thrown upon the liver, which is kept in a state of greater activity. There is an increased determination of blood to that organ, and an augmented flow of bile. This hyperæmia of the liver is common in the case of new arrivals, who, just off a sea voyage, are in vigorous health and in high spirits. Excited by the novelty of their surroundings and the profuse hospitality of friends, they consume a greater amount of carbonaceous material in the shape of meat and drink than they were accustomed to do at home, and the liver has soon extra work to do. The increased secretion of bile usually declares itself in a violent attack of diarrhœa. If the bile does not pass off in diarrhœa, there is nausea, sickness, and headache, constituting what is usually termed a bilious attack.

Among the older residents, who are more prudent in their diet, and whose habits of living have become adapted to the climate, it cannot be said that the daily secretion of bile is greater, or that the liver acts vicarious of the lungs in the decarbonization of the blood. Although in hot climates the surrounding temperature be high, there is no increase in the bodily heat of a person in perfect health, and the body is able to support a temperature considerably above that of the blood. By free perspiration and rapid evaporation the body is kept at the normal temperature, and only when the air is saturated with moisture, retarding evaporation, does the heat become oppressive. A temperature of 110° Fahrenheit in a dry climate like that of Egypt or Arabia is more easily endured than 86° or 88° in a moist atmosphere. When the rainy season happens to be the hot season, the combination of heat and moisture is most trying to the European, and continuing, as it does, for months, soon causes anæmia. The healthy colour of the skin is succeeded by a dusky brownish tint, which, however, is not icteric. It is often attributed to some kind of jaundice, but is really a deposit of pigment in the tissues, which seems to be derived from the destruction of the red corpuscles. It is the natural result of degenerated blood and the malarial cachexia. True jaundice is not at all common, although in former days, when mercury and venesection was the acknowledged treatment for fevers, skins of a yellow hue were more commonly met with than now. Amongst Europeans who have lived long in malarious districts, anæmia, with or without the brownish tint of skin, is almost universal, and it may be observed in greater or less degree in every one who has gone through a few hot seasons. Although it is possible to reside many years in the tropics without having an attack of fever, yet very few return to this country entirely free from the malarial taint. Dr Chevers, a recent writer on Indian diseases, says, "It would scarcely be an exaggeration to say that this malarial cachexia is more or less the acquired constitutional state of every one who has remained long in India." But of course this statement applies more particularly to India and to the military part of the European population, who are obliged to serve in all parts of the country and cannot choose non-malarious districts for their residence. The civilians who live in cities escape the chances of malarial fevers to a great extent, and they have also more opportunities of going to the cooler climate of the hills or even to Europe.

It often happens that the pre-existing influence, though dormant for a time, will assert itself and be roused into action by some very ordinary cause. A chill, over-fatigue, an accident, or a wound may bring on an attack of ague. When a person is for the first time attacked by malarious fever it usually takes the quotidian form. In subsequent attacks, or when the individual has removed from the malarious district, it appears as tertian or quartan. Tertian is the form in which we expect to find it in this country among Anglo-Indians and in cold climates generally; and associated with the fever, you may find splenic enlargement. This enlarged spleen, as you are aware, is very common among natives of India, very much more so than amongst Europeans. So general is it among the natives, and so associated with malaria, that before allowing a camp to be formed in any particular locality, it has been proposed to examine a certain number of natives to find out what proportion of them have enlarged spleens, and by that means to judge of the salubrity of the place. Both the spleen and stomach suffer in a marked manner in malarial poisoning, more so, perhaps, than the liver, and a hypertropied spleen is always suggestive of long exposure to the malarious influence. During an attack of fever the spleen becomes congested in common with the rest of the portal system. This enlargement is temporary, and may subside with the fever; but while it lasts, there seems to be great destruction of blood corpuscles, which may account for the exhaustion which follows the feverish attack. Sometimes there are attacks of subacute splenitis which seem to take the place of the fever paroxysm. As a sequel to repeated attacks of hyperæmia, the spleen may get permanently enlarged; but usually the chronic enlargement is a slow process, and goes on, attended by a marked malarial fever, so slight in its symptoms as to escape notice altogether. Ascites may follow, but it rarely does so, unless the liver also is diseased.

Remittent fever differs from intermittent in there being no complete cessation or intermission in the febrile phenomena, but merely an abatement or *re*mission. Jungle fever, Assam fever, Hong Kong fever, are names for the same complaint. It usually occurs in the hottest weather, and is a most formidable disease as compared with intermittent. So rapid is it in its effects, that it is often confounded with sunstroke. I have known persons to die within forty-eight hours, never having had any *re*mission of the symptoms. At times it assumes a typhoid character and is difficult to diagnose from true typhoid. It is by far the most fatal form of fever, and is the cause of the large death-rate which is registered under the head of fever. In India over 3,500,000 died in 1879 from this fever.

With remittent fever there is always tenderness over the liver, which does not, however, amount to hepatitis, and sometimes jaundice. There is often considerable congestion and inflammation of the stomach and duodenum, with vomiting. When jaundice *is* present, it seems to depend, not so much upon deficient secretion, as on the reabsorption of the bile, which is hindered from passing from the liver by congestion of the common bile duct and duodenum. The affection of the duodenum may become chronic, and is often a prominent symptom with the victims of remittent fever who return to this country. This state is sometimes taken for chronic hepatitis, a condition requiring a very different treatment.

The liver may become enlarged, as well as the spleen, after repeated attacks of fever. In a state of passive congestion during the attack, it diminishes as the fever passes off, but will enlarge and diminish with every subsequent attack. If the patient remain in a malarious neighbourhood the malarial cachexia is established, and the liver remains permanently enlarged, torpid, and with diminished secreting power. The only hope of relief at this stage is in a return to more temperate climes, and a large proportion of Anglo-Indians are invalided for this. You find them suffering from general dyspeptic symptoms, loss of appetite, frequent headache, irregular action of the bowels, nausea pretty constant, vomiting in the morning of a clear tenacious mucus, and often bile. They will tell you that they have a constant sense of fulness on the right side, as though that side of the body were larger than the other. This latter symptom is found with passive congestion, not with inflammation. The urine is loaded with lithates, and there is great depression of spirits. There is sometimes eczema, and gout is apt to be developed. There is also more or less anæmia.

Another result of the malarial cachexia is *atrophy* of the liver, which often follows from the same cause as chronic congestion, namely, intermittent hyperæmia accompanying attacks of ague, or, it may occur after a single attack of remittent, if the hepatic symptoms have been overlooked. In every case the subject of the contracted liver is, so to speak, saturated with the malarial poison. Of course this atrophy occurs quite apart from any abuse of alcoholic stimulants, and may occur in young children, when it is probably hereditary. The first symptoms resemble and are often treated as indigestion, but there is always a certain amount of uneasiness in the right side, and the patient rapidly loses strength. The countenance is sallow, the eyes have a jaundiced appearance, there is loss of appetite, disturbed sleep or great wakefulness, dry cough, urine dark coloured, sometimes loaded with cholesterin, skin harsh and dry, rarely perspiring, and very sensitive to cold. Later on there is ascites, and the abdominal veins are enlarged. These cases receive some benefit by returning to this country, but they rarely recover. They are often attacked with diarrhœa when they reach this country, with a deficiency of bile in the motions, and defective assimilation of food. The patient becomes rapidly emaciated. They are also very liable to attacks of pneumonia. The same fact has been noted in the malarious districts of Italy.

Pneumonia is a not unfrequent complication in both remittent and intermittent fevers. This fact was noted by Morehead, who named it *febrile* pneumonia.

Asthma sometimes follows an attack of fever, and even without the fever it may develop after long exposure to the paludal poison. In many parts of the East it is common both amongst Europeans and natives.

With the malarial cachexia, albumen in the urine is not uncommon, attended with dropsy of the lower extremities. This is not necessarily a grave symptom, as the albumen speedily disappears under improved climatic and hygienic surroundings. During an attack of fever, the kidneys partake of the general visceral congestion, and albumen appears in the urine. There is always a tendency to recovery, unless the congestion and inflammation be very intense, or the sufferer remains exposed to the malaria, in which case the kidneys may undergo the changes characteristic of Bright's disease.

A peculiar periodic *hæmaturia* is sometimes met with. The urine does not contain actual blood corpuscles, but hæmoglobin, and a colouring matter which seems to come from bile. There is usually hepatic disturbance at the same time, and anæmia.

In the case of *females*, the malarial cachexia, besides the usual phenomena, frequently shows itself in the form of menorrhagia, which is often intermittent. There is also a tendency to post-partum hæmorrhage, as well as to a feverish attack which comes on a few days after delivery. Years after the patient has returned to this country, the condition of pregnancy or parturition may serve to bring on an attack of fever, or the confinement may be followed by profuse hæmorrhage. *Neuralgia*, from its periodic character, may be attributed to malaria, and it sometimes occurs with simple intermittent fever. It is often very severe when attacks of fever occur in this country after a residence in the East. The pain is most felt in the occipital region, and after the severe pain has passed there remains a feeling of soreness all over the scalp. But neuralgia is much oftener met with in Europe than in the East.

There are many minor phenomena which indicate the presence of malaria, and from which the residents in malarious districts suffer, both in Europe and in the tropics. One of the most common and most annoying is a restless sleep, a disposition to awake always at the same hour, and an inability to go to sleep again. Another symptom is pain in the limbs, chilliness, pale evacuations without jaundice, sometimes constipation and vomiting. Rheumatism is often very troublesome, the pain coming in severe paroxysms. The sciatic nerve is a favourite point of attack. A stabbing pain in the hepatic region, evidently neuralgic in character, often causes needless alarm. All these symptoms have a tendency to periodicity.

I have been speaking hitherto of the malaria of hot climates, and if I am not taking up the time of the Society too much, I should like to say just a few words about the malaria of Italy. Intermittent fever is prevalent in many parts of Italy, notably in the Maremma, a marshy district which stretches from near Leghorn to Civita Vecchia, in the Pontine marshes at the mouth of the Tiber, at the lower end of the Lago di Garda, and various other localities. The woodcutters who are employed in the forest ground of the Maremma during winter are obliged to retreat to the mountains in summer on account of the malaria. I have seen these men, living 4000 feet up in the mountains, suffering from attacks of tertian and quartan ague, which they had contracted on the plains, and who presented all the appearances typical of the malarial cachexia. The fever usually takes the tertian form. English visitors do not seem to be often attacked by this fever. There is not much to attract visitors to the malarious districts, and the fear of catching the fever keeps them away. During the autumn of 1883, when the valley of the Po was flooded, and transit was partly by boat over the breaks in the railway, there were a few cases of ague among visitors. I have also known it occur in the case of sportsmen, who had been shooting in the neighbourhood of the marshes.

Remittent fever occurs in the more arid districts, and in the Roman Campagna; and during the construction of the railways throughout Italy, it was very prevalent, and proved fatal to a great many of the workmen employed in the undertaking. What the Italians call "Febbre perniziosa" is probably remittent, although the name is undoubtedly often applied by them to cases of true typhoid. The late Dr Aitken of Rome described a fever which he met with in that city, which by his description seems to have been almost identical with what in the East is called simple continued fever, and is evidently of malarial origin. The illness usually lasted about fourteen days, and there was always a crisis on the seventh day, when the temperature fell nearly to normal. Many of the subjects of this fever in Rome declared that they had not been without the walls, and so could not have imbibed the malaria of the Campagna. At that time it was quite possible to encounter the malarial poison inside the walls. Large excavations for building purposes were going on on the Esquiline Hill, a part of Rome which had not been disturbed for centuries, and a new street was being driven through an old part of the city, with extensive cuttings and disturbance of the soil; and as English visitors are sure to be found wherever excavations are going on, there was every opportunity for the malaria to seize its victims. This form, as described by Dr Aitken, might be called "Roman fever," but what usually gets the name is typhoid. The name "Roman fever" being suggestive of malaria, is, I suppose, the reason why convalescents from typhoid returning from Rome with a report of an attack of fever there are so often treated as if they had had malarial fever. It is usually a matter of some difficulty, even in this country, to diagnose a case of typhoid, but where there is a possibility and a probability of malarious surroundings, as in the tropics or in Italy, it is often very puzzling, from the similarity in the symptoms to what is called "bilious remittents." But the typhoid of Italy seems to differ somewhat from the same disease as seen in this country. Although hæmorrhage from the bowel is a complication which is often fatal, I do not think that the intestinal lesions are so pronounced or occur so frequently as in this country. The diarrhœa is often insignificant, and there is sometimes obstinate constipation. There is nearly always a relapse, the patient being rarely fit to leave his bed for six weeks. In fact, I do not remember of ever seeing a case of typhoid in Italy without at least one relapse. Whether or not malaria plays any part in this disease I cannot say for certain, but I am inclined to believe that it does.

The ordinary intermittent is less amenable to treatment than the same disease in the tropics, and the usual remedies are not so effectual, and there is a great tendency to a return of the fever for many years afterwards.

In conclusion, I beg to thank you for your kind attention to the paper which I have just read, which does not claim to have advanced anything very novel, but is merely my own observations of malaria in countries where it prevails, and where, I may say, the best part of my life has been spent.

Surgeon-Major Black pleaded for more exact definition of terms in speaking of malarious diseases. He thought the term malaria should be restricted to diseases of the intermittent type. Instead of ascribing the occurrence of the diseases to certain states of the atmosphere, it would be better to adopt the germ theory to explain them. This would simplify matters, and would facilitate treatment. In Hong-kong there were marshes of considerable extent which had been drained, but the subsoil water from the hills was found to be the cause of outbreaks there still.

Professor Grainger Stewart said that he set a high value upon such papers as that of Dr Dods, embodying as they did the results of large experience in regard to diseases seldom met with in home practice. He thought that the evidence in favour of Klebs and Tommasi-Crudeli's observations was more complete than Dr Dods would seem to admit. There were four points to which he wished to allude, and that rather in the way of asking questions than of enunciating opinions. The first was the albuminuria sometimes found in people who have lived in an ague district. It seemed to him that there were two forms of it. In the one there was an ordinary inflammation of the tubules, not of a very severe kind, attended by little dropsy and few tube-casts. These cases went on pretty persistently, but not acutely, and tended to get well. The second was a form in which no tube-casts were to be found and no trace of dropsy, where apparently the albuminuria was functional rather than organic. He should like to ask Dr Dods what experience he had as to these two conditions, and the respective values of arsenic and quinine in their treatment. Next, he had again and again met with patients and with records of cases in which paralytic symptoms were attributable to malaria, and yielded to antimalarious treatment. He had brought some of those which he had observed under the notice of the Society about fifteen years ago. Could Dr Dods supply them with more facts as to the degree of frequency with which the paralytic complications occurred? There was, thirdly, a peculiar form of disease he had sometimes met with in old Anglo-Indians. They exhibited symptoms like those spoken of by Dr Dods in describing the diarrhœa with colourless discharges. This was a very distressing disease. He had seen the patient going on steadily from bad to worse, and ultimately dying. The case looked somewhat like pernicious anæmia, the blood deteriorating in a marked manner, but the cause was evidently the constant diarrhœa. Three or four times a day a white creamy material was discharged, sometimes frothy, sometimes not, but evidently deficient in bile. He remembered one patient improving under large doses of bismuth and careful regulation of diet, but in the end he went wrong; and he (Professor Stewart) had never yet been able to save a case of this kind. Lastly, he wished to mention a peculiar manifestation of malaria —a very interesting and satisfactory one to meet with, as it proved very amenable to treatment. He saw with Dr Gairdner of Crieff a gentleman who was supposed to have phthisis. There was some roughness of breathing at one apex, along with fever and a peculiar barking cough. The fever seemed too great for phthisis. After careful examination, he found reason to concur in Dr Gairdner's opinion, that there was not sufficient organic disease to account for the symptoms. He ascertained, on inquiry, that the patient had been employed as an engineer on one of the West Indian railways, and although he had never had an attack of ague, he had been exposed to marsh poison. He was accordingly put on large doses of quinine, with the result that in a few days his cough and fever had disappeared. He wished to ask Dr Dods whether such cases were of frequent occurrence in malarious regions.

Dr James asked Dr Dods if he had any information bearing on the antagonism which was supposed to exist between malaria and phthisis. This antagonism was said to exist, inasmuch as phthisis was found to supervene on the disappearance of malaria. This might be explained on the ground that drainage got rid of the malaria, and overcrowding brought on the phthisis. Some held that the malarial poison was antagonistic to the bacillus. Others thought that it was the rendering of the lung more functional that prevented the phthisis.

Dr Felkin held that the cause of the malarial process was a germ. He had examined a number of cases abroad and since his return, and had almost invariably found this germ, and he was inclined to believe it was the germ found by Crudeli. He believed it was best to examine in the cold stage. He had missed it in the hot. He thought it could be transmitted from the father to an unborn child, and hoped to bring before the Society three or four cases to show this. In one case of a fœtus that had attacks before it was born, he was having the spleen examined. In regard to malaria in dry places, the influence of the subsoil water was apt to be forgotten. It was easy to understand the occurrence of ague in the Sahara when they remembered that at a certain depth there were impermeable rocks, and the subsoil water might be raised to within a few inches of the surface. Bearing on Prof. Stewart's remarks, he had three patients who fought in the Soudan and suffered from ague. They all had hæmoptysis, and were cured by large doses of quinine or Warburg's tincture, of which he had had tabloids made to obviate the nausea that sometimes followed its use. As illustrating the influence of masked malaria on the weak points of a man's system, he mentioned the case of a veterinary student, who had been seven years in India, and had his first attack in the Bay of Biscay. When working for his examination he suffered from brow ague. In the holidays, when riding he had sciatica, and latterly he had been subject to paroxysmal attacks of orchitis, not from gonorrhœa, but from the excitement attendant upon his engagement.

Dr Dods said, in reply to Dr Black, that if the germ theory were the true one, no explanation had been given as to the occurrence of the germ in particular localities only. And there was nothing in the appearance of the so-called "bacillus malariæ" which was pathognomonic of paroxysmal fevers. It was just as likely that these organisms found in the blood were the result of the fever as the cause. There were no marshes in Hong-kong to account for the fever there. He had never seen casts in the albuminuria to which Professor Stewart had referred. In old-standing cases of fever he thought that arsenic was preferable to quinine. In the fever of Italy more benefit was to be got from arsenic. The diarrhœa of which Professor Stewart had spoken was generally associated with atrophy of the liver. He had never seen a case recover. As to the antagonism of phthisis, he could not speak with certainty, but most of the cases of phthisis he had seen amongst natives occurred in the families of well-to-do people and generally town residents, while ague was most common amongst the agricultural classes, and phthisis was not. Dr Felkin had drawn attention to the fact of there being generally sub-soil water where malaria was rife, but this could not be the case in Hong-kong, where fever nearly always occurred after a cliff (which might be perpendicular) had been scarped. He had never seen hæmoptysis in any of the cases.

Meeting VIII.—May 2, 1888.

Dr John Smith, *President, in the Chair.*

Original Communications.

1. A CASE OF MULTIPLE NEURITIS IN A WOMAN, OF COMBINED SYPHILITIC AND ALCOHOLIC ORIGIN; TREATMENT BY ELECTRO-MASSAGE; COMPLETE RECOVERY.[1]

By Andrew Smart, M.D., F.R.C.P. Ed., Assistant Physician, Royal Infirmary, Edinburgh.

The disease to which the term multiple neuritis is now applied was first, though imperfectly, described by Duménil in 1864. But the subject dropped out of notice until 1876, when Eichhorst reported a case under the name of "Acute Progressive Neuritis," in which he found peripheral nerve degeneration without change in the brain or spinal cord. Three years afterwards, Eisenlohr and Joffroy brought forward fresh cases of the disease, followed in 1880 by contributions by Leyden, and in the following year by Lancereaux and Grainger Stewart. The last-named observer reported three well-marked cases of the disease, in one of which the autopsy showed the nerves only to be diseased, the brain and spinal cord being almost healthy.

The pathological changes found in the diseased nerves consisted in a more or less complete atrophy of the nerve fibres, with thickening of the perineurium, and accumulation of fat cells between the bundles; together, as pointed out by Leyden, the deposit of pigment around the bloodvessels, which, as he thinks, gives evidence of an inflammatory hæmorrhagic condition of the tissue between the nerve fibres, which by compression leads to their atrophy. (See Drawings). The disease described by each of these observers is characterized by a combined group of associated symptoms occurring together or in quick succession, which, until demonstrated to the contrary by the clinical facts and post-mortem evidence referred to, were believed to originate in spinal disease. These symptoms, in many respects closely resembling others induced by disease of the brain or cord, or both, are now becoming better known as pathognomonic of certain degenerative alterations occurring in the peripheral nerves, which, if unarrested, tend to the complete destruction of their functions. This disease is one occurring mostly in women between the ages of 30 and 50 years.

Although the correct pathology of this disease only began, as I have just said, to be appreciated after the publication of Duménil's

[1] It formed the subject of a clinical lecture in the Royal Infirmary in February 1888.

observations in 1864, yet so early as 1822, Dr Jackson of Boston drew attention to certain disorders of the nervous system which he

FIG I

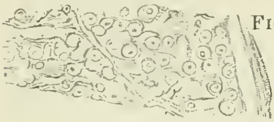


FIG 2

FIG 3

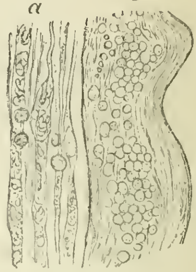


FIG 4

FIG. 1.—Transverse section of sciatic nerve in multiple neuritis (alcoholic), showing acute degeneration of the nerve fibres (parenchymatous degeneration). The area of the atrophied nerve fibres is occupied by tracks of branching connective tissue enclosing the space which had been occupied by nerve fibres, and in some of which healthy fibres still remain.

FIG. 2.—Section from a less affected bundle from musculo-spiral nerve, showing a less complete stage of degeneration.

FIG. 3.—A fasciculus from median nerve in which the concentric growth of the fibrous tissue has invaded the whole area of fasciculus, resulting in complete destruction of the nerve fibres. (Buzzard modified.)

FIG. 4.—Longitudinal section in multiple neuritis, showing degeneration of nerve fibres,—(*a*) the myelin broken up into masses, globules, and granules, with (*b*) accumulation of leucocytes in nerve sheath. (After Leyden, modified.)

connected with the drinking habit as their cause, having symptoms identical with those which we now recognise as distinctive of multiple neuritis. Jackson's views appear to have been lost sight of until 1849, when they were revived by Huss of Stockholm. But it was not until the publication of Dr Wilk's cases of drunkard's paralysis that professional attention was fully directed to the existence of a disease which could be properly designated "alcoholic paralysis." The cases of this disease, which have since been reported by careful observers of this and other countries, tend to corroborate the accuracy of previous observations alike as to the etiology and pathology of the disease. I have brought forward the present case, which I believe to be primarily of syphilitic origin, although doubtless aggravated by the patient's intemperate habits, partly as a contribution in support of these views, and partly to recall attention to the operation of other causes in the production of those degenerations of peripheral nerves which present their effects to us in the shape of a definite palsy. It is probable, if not positively certain, that, with a growing and more intimate knowledge of many neuroses whose pathology is still but imperfectly made out, we shall have to admit the potent agency of many other causes besides those of alcohol and syphilis in bringing about these degenerations. Is, for instance, the palsy of lead poisoning not a multiple neuritis? And may the same not be said of every kind of metallic poisoning? Is the paralysis of diphtheria not a peripheral nerve degeneration due to the specific blood poisoning of the disease? Again, may not the nerve disorders associated with advancing mellitic diabetes be viewed in the same light? the glucose in that case acting as the toxic agent. The complete loss of the knee-jerk occurring in the advanced stage of that malady is very suggestive, as the absence of the knee-jerk is the most constant and reliable diagnostic mark of a multiple neuritis. These instances are only named as suggesting a much wider range of possible causation in operation than we have hitherto accustomed ourselves to suppose; and as tending to show that a multiple degenerative peripheral neuritis may be the underlying pathological condition common to a large variety of variously modified disorders, and by a reference to which the neurotic symptoms developed during their course can best be explained.

The patient whose case is now under consideration was admitted to Ward VI. of the Royal Infirmary of Edinburgh in November 1887.[1]

Previous History.—She is a widow in her thirty-seventh year, her husband having died a few months prior to her admission. She had been married about eleven years, and during that period had borne five children, had two miscarriages, and during the whole of her married life had suffered from menorrhagia.

Four of the five children died in infancy, the survivor, a boy of ten years, being in delicate health.

On account of the frequency and continuance of her menorrhagia she was often confined to bed. On getting out of bed on one of these occasions, her husband having died during the interval of her seclusion, she experienced some difficulty in walking, and having on the same day visited the cemetery in which her husband was interred, whither she had driven, she on leaving the cab fell to the ground, then feeling unable to walk or stand. On reaching her house she again took to bed, where, with only occasional attempts to go about the house, she remained until brought to the Infirmary. She explains that, besides the difficulty of walking, two months before her admission she began to experience feelings of cold and numbness, with tingling and pricking sensations in the feet and legs, and later on pains of a darting and burning character which occasioned her much distress—the pains being aggravated by recurring exacerbations; and she stated that, shortly before going to the Infirmary, similar sensations had come into the hands and fingers, although without pain.

Her Condition on Admission.—She cannot walk or stand, even with the assistance of others, and when any attempt is made to put her on her feet she makes a great outcry with tears. Her decubitus in bed is on the side with the knees drawn up, the

[1] W. A. Turner, M.B., C.M., acting as Resident Physician.

feet rigidly extended, and the toes curved under the soles. By much and apparently painful effort she extends the legs, but is unable to flex the feet or point the toes. Slight pressure over the tibial nerves, especially the anterior, elicited the expression of acute pain. The cutaneous hyperæsthesia over area of distribution of anterior tibial nerve is well marked, but away from that area it becomes irregular and not readily definable. Any attempt, however slight, at passive movement of the legs is attended by apparently excruciating myalgic pain. This is markedly so when the calf muscles are handled. Her dread of pain obliged us repeatedly to defer our examination, and kept the patient persistently in the same position in bed. Owing to her perturbation when we approached her we could not get the patient sufficiently to fix her

Fig. 5.—Usual attitude of the hands and feet, showing paralysis of extensors of the wrist and flexors of the ankles.

attention so as to be able to localize sensory impressions, and the cutaneous tactile sensibility could not on that account accurately be ascertained.

The muscles of the legs are not more atrophied than would be accounted for by their disuse for two or three months. There is no œdema. Plantar sensibility is deficient, but when, at a later stage, she was put upon her feet she said that she felt the ground perfectly under them; but if, for an instant, left unsupported in that position she sinks to the floor. Knee and ankle-jerk are abolished. The sphincters are not impaired, and the bladder and rectum are acting normally. The electrical test elicited a feeble "reaction of degeneration." Her mental condition is marked by defect of memory, extreme emotional depression, and incapacity for endurance. There has been no delirium, delusions, or hallucinations. Her sleep is fairly good, and she is not disturbed by apprehensive dreams.

She now complains of coldness of the hands, with pin and needle sensations. She is always rubbing them. The mobility and tactile sensibility of the hands are defective, and she is becoming awkward in using them. There is commencing wrist-drop; vision normal; pupils fairly responsive to light and accommodation; no headache; no strabismus; absent myosis and nystagmus.

Treatment and Progress.—I was not, at the time it became necessary for me to consider the treatment to be adopted, in possession of that part of the patient's history which afterwards disposed me to suspect a syphilitic cachexia. The knowledge of a prior history of that character, although not materially affecting the course of the disease, would have doubtless determined me to adopt a decidedly specific line of treatment. The patient's unreliable memory as to her antecedents, and the absence of trustworthy information from an extraneous source, put me at a disadvantage. It was not, in fact, until her recovery was assured that I became aware from an authentic quarter that my conjecture as to syphilis was well founded. The treatment adopted, however, proved successful beyond expectation.

The patient was deprived of all stimulants, even to tea and coffee. Her condition of anæmia indicated the need of iron, which was given as the valerianate with strychnia and arsenic. But the remedy from which I expected most lay in the application of massage—in the first instance by gentle hand friction suited to her capacity to bear it. This she at first resisted, as she did every method of treatment, but was reconciled by early and continuously experiencing the benefit derived from it.

At a further stage I combined electrolysis with massage by means of a metallic roller which I had made to be readily adjusted to any battery as an electrode. By this arrangement the roller could be applied with any degree of pressure, and freely moved over the paralyzed limbs, thus combining at the same moment the faradic current with the massaging process. This process daily became more agreeable to the patient, discrimination being exercised as to the strength of the current and the amount of pressure applied. Improvement from the beginning of treatment was unequivocal and steadily progressive, and only retarded by the necessarily slow recuperation of nerves, which had doubtless undergone partial structural changes. An added delay arose from a degree of contracture of the calf muscles induced by the loss of the normal antagonism derived from their opponent muscles of the front of the legs.

In consequence of these drawbacks the patient's earlier efforts at locomotion were laborious, and characterized by fixity of the ankles and curving under of the toes. Progression, consequently, for a long time was effected by a shambling, shuffling movement on the outer edge of the feet, which were kept widely apart, her eyes being apprehensively fixed on the ground. She had, of course, to be assisted by the nurse. Her movements were not at any time inco-ordinate. This absence from ataxia is important, as contra-distinguishing multiple neuritis of the female from the alcoholic ataxia occasionally met with in the male, and as also differentiating this form of paralysis from paraplegias of spinal origin.

After three months' treatment, carried on under great discouragement, the patient so well recovered the use of her legs as to be able to walk unassisted the whole length of the long corridor, the power to flex the feet and extend the toes gradually returning. She had two months previously recovered the use of her hands. After three weeks more, passed at the Convalescent Hospital, she returned to her home, where I afterwards saw her attending to her household work in still improving health.

I have just spoken of a distinction in the diagnosis between the paralysis of a peripheral neuritis and that of a spinal paraplegia. It might be useful to extend this comparison of symptoms to some other diseases in which the symptoms are apt to be confused in diagnosis with those of multiple neuritis.

1. In the first place, it is necessary to distinguish it from hysterical paraplegia. In that case it is scarcely needful to do more than to remember that the knee-jerk in multiple neuritis is always absent, but in purely hysterical conditions it is always present.

2. In paralyses connected with the brain the tendon reflexes are exaggerated; there is also tendency to muscular rigidity, and the electrical conditions are unchanged.

3. While pupil symptoms are not present in multiple neuritis, they are diagnostic of locomotor ataxia of spinal origin. There is, in addition, in the latter the characteristic inco-ordination. And there is also the absence of qualitative electrical changes.

4. Acute ascending paralysis may be distinguished by the absence of any marked sensory phenomena. And the electrical conditions are unaltered.

5. Paralysis connected with myelitis is early associated with bed-sores, and with vesical and intestinal complications—not present in multiple neuritis.

6. In spinal meningeal disease there is extreme back pain, characteristic retraction of the head, and muscular twitching.

7. In the paralysis of poliomyelitis the muscles of the trunk are first in order of succession paralyzed after the legs. In multiple neuritis it is the extensor muscles of the forearm that are next paralyzed after the legs. When the paralysis in poliomyelitis passes from the trunk to the arm it first attacks the intrinsic muscles of the hand. The two diseases are further differentiated

by the absence of any sensory disturbance in the progress of poliomyelitic paralysis.

Prognosis.—In conclusion, it will be evident that early and accurate diagnosis is of the utmost consequence; and that the likelihood of recovery in any case will depend upon the amount of pathological change which may have taken place in the nerve fibres. Degeneration may have reached a stage in which structural recovery is impossible. But, notwithstanding, active treatment should be perseveringly followed in every case, not only in the hope, but with the certainty of arresting the further extension of degeneration to yet unaffected nerves. It need hardly be said that where alcohol is the cause of the disease, its absolute suspension is necessary; and that a similar course should be adopted in the removal in all other cases of known predisposing conditions.

My experience of the advantage to be derived from massage leads me strongly to advise its employment in addition to other remedies, and to combine it, as I did in the present case, with faradic electrolysis.

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The President said he did not know, if the tissues of the nerve fibres themselves were disintegrated, how their function could be restored. He could understand it if only the tissues surrounding or connecting the nerve bundles were affected, but if the histological elements of the fibres themselves were replaced by degenerated structures, it was not easy to see how the nervous power could be restored. It seemed more probable that any remaining elements took on increased action than that new ones were formed.

Dr James Ritchie remarked that from the notes read it would appear that Dr Smart had not seen this interesting case till somewhere about two months after the disease commenced. As *progressive multiple neuritis* usually begins as an acute affection, Dr Ritchie desired to know if in this case there was an acute stage? Was there any tenderness over the affected nerve trunks? He desired also further information as to the plantar cutaneous sensibility, because in this disease the cutaneous sensibility of affected areas was usually much diminished, and might be entirely abolished. Was there interference with nutrition of nails?

Dr Smart, replying, said he agreed with the President that if the degeneration of the nerve substance were complete, regeneration became impossible, but he looked on this point as he did on the regeneration of muscle. If a proper stimulus were applied to a muscle undergoing simple atrophic degeneration, it may be completely restored so long as a certain proportion of its normal structural elements remain entire. He considered the same held good with regard to nerves. With reference to Dr Ritchie's questions, he thought the patient came to him in the acute stage. There was great tenderness over the trunk of the anterior tibial, which was the nerve most affected. When he said there was sensibility over every part of the sole of the foot, he did not mean to imply that it was complete. There was a diminished sensibility, whether due to the rest in bed or to the affection of the nerves in the neighbourhood he was not sure. As to the nails, they were so overgrown that it would have been impossible to say whether their nutrition was interfered with.

II. THE DIAGNOSIS OF EARLY PHTHISIS BY THE MICROSCOPE.

By Francis Troup, M.D. St And., L.R.C.S. Ed., M.R.C.P. Ed., Assistant Medical Officer, Longmore Hospital for Incurables, Edinburgh.

It has long been the belief of the laity that consumption is an infectious disease, and this persuasion has acquired justification and scientific foundation from the discovery, simultaneously made by Koch and Baumgarten, of the specific bacillus of tubercle, and the facts of experimental pathology leave no doubt as to this parasite being the cause of tuberculosis.

Some, however, speak as if after all it may not be the bacillus itself, but some extra-bacillary matter secreted or excreted during its growth, which causes the mischief. This supposititious matter, however, seems so identical with the bacillus that it is never noxiously operative save in its presence, and must be transmitted from generation to generation of bacilli in pure cultivations. If this chemical virus really does exist, it should therefore be easily demonstrable in those pure cultivations, and should be searched for in them alone. To examine sputum and find in it alkaloids of a poisonous nature is not scrupulous enough; there are many micro-organisms which flourish side by side in expectoration, and it is random work to say that any noxious matter detected is the derivative of one microbe rather than another.

It is admitted on all hands that inoculations of pure cultivations of tubercle bacilli, or of tubercular stuff containing them, into animals susceptible of the disease, cause, in a definite time and in a definite series, certain morbid phenomena, which are summed up under the name of general tuberculosis; but how is a spontaneous localized tuberculosis, a chronic phthisis, for instance, brought about? We speak in a vague way, and vary the formula of our ignorance by saying that the general disease is caused by the violent intrusion of relatively enormous masses of the bacilli into the organs of a healthy animal effected by the inoculations; that the entrance of only a few into sound lung-tissue causes a phthisis. We talk of "immunity," "individual predisposition," "morbid weakness of tissues," "heredity," and so on, to account for the fact that some people fall victims to phthisis and others do not. It is matter of common experience to know of whole families swept off by this disease, strong and weak alike; or to see among the children of the same progenitors the most herculean of the flock succumb, and the weakling, with compressed chest and bent spine, spared.

We speak also of traumatisms of the lung, catarrhs, and diverse infiltrating inflammatory processes, as if they were necessary preparatory lesions for the reception and colonization of the bacillus. Daily experience again teaches us that those conditions are not indispensable; no tubercle bacilli will be found in the bronchial secretions of a non-phthisical person, however long a catarrh may have endured; and, on the other hand, in a supposed trifling cold of only four weeks' duration, happening in a person up till then absolutely free of demonstrable lung disease, I have detected bacilli and fragments of elastic tissue.

It is supposed that the air contains in abundance the spores of the bacilli, or the bacilli themselves, in a condition capable of growth; that they are inhaled, and that a damaged pulmonary tissue affords them a suitable nidus, and becomes the *atrium morbi.* Now there are certain normal arrangements of the body which afford effectual protection against the penetration and multiplication of bacteria. This is well illustrated by the fact that many years of extrauterine life are needed to pigment the lungs with the particles of carbon so richly suspended in the air we breathe. Baumgarten has tried to make cultivations from portions of tracheal and bronchial mucous membrane and lung-tissue of healthy animals living in his laboratory, immersed among bacteria of all kinds, and failed to grow fungi of any sort, thus showing that practically the pulmonary apparatus was free of micro-organic life. He also placed plugs of wadding on the branders of a stove with a strong draught, soaked the flooring of the room in which it stood with liquids and solids full of virulent tubercle bacilli; in the traffic of walking over and of sweeping the floor, the dried bacterial stuff was raised up in clouds of dust. The wad stopples were thus exposed for weeks or months, and rabbits were inoculated with bits of them, which presumably should have been filled with tubercle microbes, but no tuberculosis resulted. In a small way I have made careful endeavours to find the bacilli in the breath of consumptives, but without positive result. From all this it would seem that the air really contains a very minimal quantity of tubercle bacilli in a condition capable of causing infection. Probably drying causes them to lose their original virulence and energy of growth, and they find themselves unable to take root and flourish even in favourable ground. How then can a phthisis, which is commonly supposed to be an inhalation tuberculosis, be acquired in Nature's ways when so few tubercle bacilli exist in the atmosphere in a potentially infective condition? It is true that the forced inhalation of great masses of bacilli contained in pure cultivations or in richly bacilliferous sputum will excite a lung tuberculosis, but this violent process is very dissimilar to what takes place in Nature's laboratory.

In answer to this question three factors may be supposed to play an important part. 1st, By the use of the milk of tubercular mothers of the human species and the eating the flesh and drinking the milk of tubercular animals, the specific bacillus may be introduced into our bodies. 2nd, It is not unthinkable that the bacillus itself, and not a mere disposition, may be inherited. In a recent communication from Baumgarten he informs me that he stands, as yet, almost alone in Germany in the adoption and advocacy of this view. One is free to confess, however, that the pathological rarity of demonstrable embryonic or fœtal tuberculosis militates greatly against this opinion. 3rd, Phthisis may be, oftener than we are accustomed to think, not a primary disease, but a secondary infection from pre-existing cheesy tubercular foci, it may be in some distant region of the body. Of course this supposition only throws back the question a stage, and does nothing towards its solution.

I shall not occupy the time of this Society by describing the tubercle bacillus in detail; its appearance is abundantly well known to most of us. I content myself with showing photographs of its form and grouping as it appears in sputum, and also its pure cultivation, for the beautiful specimen of which I am indebted to the great kindness of Koch. I may also mention that I have succeeded, after many failures, in growing it directly from sputum, and with no more expensive an incubator than an empty biscuit box. The magnification of the photos is 550 diameters, and it is easily seen from them that the bacilli are not so uniformly slender and regular in size as they are depicted in the drawings in Koch's *Mittheilungen*. In sputum, to which alone my remarks apply, they are often found aggregated into heaps, or lying together in twos and threes with their long axes parallel, or forming rectilineal figures of four to eight, as if they had been enclosed in a cell whose walls had disappeared and permitted their dropping out while retaining their intra-cellular arrangement. Frequently they are seen to be very much beaded—"gekörnt" is the German adjective; they then seem made up of from three to ten minute cocci cemented together into a rod. This variety is found in greatest abundance in very acute cases of phthisis with high fever and rapid disintegration of lung, and is often accompanied by another coccal or diplo-coccal organism, photos of which I show, occasionally in enormous quantities. The spit in which I have found this companion organism is generally tough, tenacious, transparent, frothy, and contains so much albumen that a very gentle heat coagulates it into a solid mass. In slower phthisis, when intercurrent inflammatory attacks happen, this coccal organism also appears often, and the bacilli of tubercle change into the beaded forms. In other cases of old phthisis, where the process is sleeping for the time, one meets in the sputum heaps of granules which, resisting decoloration by acids, I take to be ruins of bacilli. The entire bacilli perhaps stain only at their poles or in the centre, and their empty sheath is visible and wrinkled into folds very much like an ill-gartered stocking.

In staining for the bacillus I do not usually wait till the sedimentation of the more solid constituents of the expectoration takes place; time, or gentle heat as pointed out by Dr Philip, or the addition of weak alkaline solution and boiling, can bring this about; and one obtains, of course in the precipitate, a greater abundance of the bacilli, and when their numbers are small, all or any of those plans are advantageous. I generally, however, examine the fresh sputum without any preparatory treatment, and find what I look for if it is there at all. The staining properties of the bacillus remain unimpaired for very long; in one photo handed round it is seen after 31 months, and in spit four years old I find they stain as easily as they did when it was recent.

For merely diagnostic purposes the most trustworthy stains are two—the aniline-Fuchsine of the Koch-Ehrlich, and the carbol-Fuchsine of the Neelsen solution. The latter is the preferable, for it does not soon decompose, can therefore be prepared in quantity, and stains as intensely after months as it did at first, whereas the aniline water stains soon become turbid and useless. However, if one can be at the trouble to prepare the aniline water each examination, or at most after the lapse of a week or two, it is quite as trustworthy as the other. As to the microscopical requisites, I have to say that if the bacilli are numerous, and one possesses a certain amount of skill in recognising them, they may easily be found with a dry object-glass of 250 diameters, or even less; but by far the most satisfactory mode is to use some form of condenser, as Abbe's, for illumination, and an immersion object-glass, water, or, still better, oil. The latter can now be obtained very cheaply; and Mr Forgan has shown me a Leitz, one-twelfth of an inch, quite equal in its performance for diagnosis to the Zeiss one-twelfth, and at nearly one-fourth of the price of the latter.

If the bacilli are detected, their diagnostic significance is the highest possible; and they are to be found in every case of tubercular phthisis, and in it alone of lung diseases,—now numerous, now scarcer, but appearing with a persistence that will not escape the notice of perseverance and a modicum of practical knowledge in the search. Therefore a negative result in the hands of one accustomed to the examination (and every one can easily acquire the necessary skill) is also of great value diagnostically. Naturally it is only where the breaking up of the original reaction products has begun and opened a way outwards for the bacillus that it will be found. In acute miliary tuberculosis it will not be seen at the beginning, and it is here and in all suspicious cases that a thorough quest for elastic tissue should be instituted; the two methods should go hand in hand.

As to prognosis it may be asked, Has the number or size, etc., of the bacilli in sputum any demonstrable connexion with the probable course of the phthisis? With the sole exception of the constant appearance of swarms of the beaded bacilli already alluded to, I would say most decidedly No. Prognostically we are in no better position with the bacilli than without them. The one thing certain, however, is that the lung disease is tubercular where they are present. The prognosis even of a tubercular process depends on many other factors than the number of bacilli,—the amount of fever, for instance, the sweats and diarrhœas and cachexia, and the intensity of the destructive process in the lung, as measured by the amount of elastic fibre shredded off.

I shall now say a few words about it. Prior to the discovery of the tubercle bacillus, great reliance was placed on the presence of elastic fibres in the sputum as evidence of phthisis; they were considered pathognomonic of the disease, and even now I do not consider this view to be a very highly exaggerated one. In no case of phthisis will they be missed if sought for with sufficient patience. They are to be found very early in the disease, and in the most innocent looking sputum. They will be discovered before stethoscopic or percussion sounds, even when listened to by skilled ears and interpreted by skilled brains, give any other than uncertain information as to what is going on. That they precede the bacillus, mayhap for a considerable time, as daily examination of the expectoration of more than one early case has taught me, I can affirm with confidence. Therefore their diagnostic value is of a very high order, and should not be disparaged in favour of the tubercle bacillus, to which I do not think a first place should be conceded. The bacillus may be absent temporarily, not so the elastic tissue, which may be detected abundantly where bacilli are extremely few in number. For its discovery no elaborate microscopical appliances are necessary—a power of 150, or even 80 diameters is amply sufficient, and when seen the evidence of lung disintegration is unassailable.

It has been said that unless the tissue has an alveolar arrangement much stress need not be laid on its appearance in expectoration. This is a great mistake. I show photos, selected from many in my possession, from cases of undoubted pulmonary phthisis, where this alveolar arrangement is very well seen, and others where the fibres are straight and in thick fasciculi, totally destitute of alveolation. No preparatory treatment of the sputum is needed for their demonstration. One, after a little experience, is able to select the dirty-white or reddish-yellow particles in which the fibres are likely to be found. A little morsel is pressed between the slide and cover-glass, and the examination proceeded with; or a drop of a 30 per cent. solution of caustic potass may be first added. As the fibres are not very compressible, they glide to the edge of the cover-glass, and will probably be seen there in greatest abundance. Different from the bacillus, it is not easy, to one accustomed to microscopic work, to mistake these fibres or to confound other things with them. One cannot say of tissue, however, that it is only present in the spit of phthisis—other destructive changes in the lungs cause its appearance; but if seen, the supposition is strong that this is the disease with which we have to do, and the supplementary search for the tubercle bacillus will complete the diagnosis. It is also as strongly resistant to putrefaction as the bacillus, and I have easily found both in a sputum sent from the antipodes. One of the photos handed round (they all have the magnification written on the back) shows how small a quantity is sufficient to attract the attention of a practised eye. Fenwick's plan of boiling with soda and dilution with water gives in the sediment a sort of epitome of the quantity of tissue contained in any sputum. I very seldom need to resort to this process, but I show two photographs of tissue after the expectoration has been treated in this manner.

In earnestly urging the importance of searching for elastic fibre in the very early stages of phthisis, and in all cases where such suspicion is entertained, I may have unduly magnified its diagnostic position, and may have failed to rehabilitate and restore its discovery to its former, and, as I believe, proper diagnostic rank. Many may dissent from my views; of course, one man's experience, however extensive it may be, is not all experience; but I can truly say that the microscope, by revealing to me the presence of curly fibre in a sputum has, on not a few occasions, enabled me to know that a phthisis had begun when, had I trusted to my ears alone, nothing so grave and serious would have been apprehended; and conversely, to assert with well-founded confidence that other cases were non-phthisical whose signs and symptoms pointed strongly to a contrary verdict.

In conclusion I may remark, what cannot fail to interest an audience of Edinburgh medical men, that it was the reading of the late Prof. John Hughes Bennet's *Introduction to Clinical Medicine*, published nearly forty years ago, which first led me to recognise the great importance of and satisfaction to be gained by a systematic examination of the sputa in suspicious cases of lung disease. It is with the Oberhäuser microscope, recommended and furnished by him, that I have done, and continue to do, my chief work in that line of investigation; and it is with the No. 7 object-glass of Oberhäuser's system that the photographs of lung tissue exhibited this evening have been made.

The President understood Dr Troup to mean that the presence of tubercle bacillus was an absolute and conclusive proof of the existence of phthisis, but drew attention to the fact that some observers held that tubercle bacilli might appear in sputum which was really non-phthisical. In such cases, when phthisis did not exist, it was probable that the bacilli had not found a soil suitable for their development and pathogenic activity. He considered the presence of elastic fibres a very important sign, as indicating the tissue of the organ being broken up. He regarded Dr Troup's micro-photographs of value in many respects, and not the least in exhibiting the varieties in size presented by the same kind of bacillus.

Dr Woodhead said there were some points in the paper which were to him in a new light, and one of these was that Dr Troup considered that danger of infection by the atmosphere was comparatively slight. In this, however, he corroborated what had been pointed out by other investigators, referring more particularly to the investigations of Drs Haldane and Carnelly on micro-organisms in sewers and their connexion with typhoid. These gentlemen have come to the conclusion that it is rare to have the disease propagated by micro-organisms carried through the air from drains, and that in those cases in which the drainage is said to be bad, the milk or water is usually contaminated, and is the medium by which the disease is propagated. However, they cannot be taken as a hard and fast line for us, because they give certain cases in which the air might become strongly impregnated with micro-organisms from other sources; and if this is true of such an affection as typhoid, he thought it might be true of phthisis also. Dr Troup spoke of weakness of the lung tissue as not being indispensable to the propagation of the disease by the tubercle bacillus. He should be inclined to receive this with some reserve. There were cases in which children had died from catarrhal pneumonia after measles, diphtheria, and other conditions where, in the catarrhal pneumonic patches, one has been able to demonstrate in enormous numbers the bacilli. One may be told that these are only secondary, the dead tissue forming a nidus in which the organisms grow; but if these are present in such numbers, and have any causal relation to the disease, they must ultimately prove centres of infection. This was an extreme case, but it would hold good in a lesser degree of slighter lesions. Then again they must look to the lymphatic glands. A large number of organisms must make their way into the lymphatics and lymphatic glands just as dust does. So long as the organisms could be destroyed in the glands, so long was there no tuberculosis, but when once the glands were not able to cope with the bacilli, they began to caseate, and in the lymphatic area in connexion with it they found tubercular changes going on. He agreed with Dr Troup as to primary and secondary tuberculosis. Recently the records of a couple of interesting cases appeared in Virchow's *Archiv* of chronic phthisis with cavities at the apex, where the lungs had remained in this condition for a considerable time. Hæmorrhage occurred, and a sudden outbreak and inoculation of the whole system with this bacillus, and general tuberculosis as a result. In both cases the incubation period was that mentioned by Koch, about twenty days. The reporter held that in such cases the bacilli made their way into the white corpuscles and urethra, carried principally by the bloodvessels and partly by the lymphatics into the system, general tuberculosis being set up. A very interesting point brought out by Dr Troup was the difference in size of tubercle bacilli as observed even in sputa, and this was particularly well shown in the photographs handed round. The difference in size of the bacilli had been a matter of dispute, and had been greatly emphasized by some observers, and some had even said that this difference was sufficient to indicate that the tuberculosis met with in man and that found in animals were not the same, but Dr Troup's photographs set the matter at rest, inasmuch as the bacilli differed very materially in size, and that in the same sputum. In connexion with the inheritance from the mother to the child, he did not think that as yet there had been any case of proved inheritance, and he was very sceptical on the point; they must take into account what Dr Troup had mentioned, the very great probability that the child might be infected through the mother's milk. Although Watson Cheyne pointed out recently that there have been a number of observations on the secretion of micro-organisms in milk, he did not think it probable that tubercle micro-organisms could make their way into the milk unless the gland itself was tubercular, a condition frequently met with in cattle. He could find no record of a tubercular mammary gland in the human subject. Still it was *possible*, though not probable, that the organisms might be secreted in the milk, and this should be borne in mind in considering this subject.

Dr James Ritchie asked, with regard to the rabbits which in Baumgarten's experiments were reported not to have taken tuberculosis, whether they had been killed and examined. Different animals showed different degrees of susceptibility to the poison. Calves fed with tuberculous milk were never affected; guinea-pigs quickly succumbed to the disease; rabbits apparently escaped, but upon examination after death their intestines were found to be studded with tubercle. If the presence of elastic tissue in the sputum preceded the bacillus, Dr Troup's observation was a most important one.

Dr Gillespie asked if Dr Troup considered the tubercle bacillus to be the cause of phthisis, and the sole cause of it? He had not gathered this from anything that had been said.

Mr Cathcart asked why, if the bacillus did not appear in the sputum till after breaking down of the lung tissue had taken place, the elastic fibres should be found before the organism. He thought that breaking down must have occurred before the fibres appeared, and could not see why the one should be found before the other. With reference to mammary tuberculosis, he was sure that cases had been recorded as having occurred in the human female. If the air of sewers did not sometimes cause affections such as typhoid, it was difficult to account for those cases of illness in a family which disappeared after defective drainage had been put right. There were numerous instances of this, and he believed that erysipelas was sometimes communicated through the atmosphere.

Dr Hare took exception to certain remarks in the paper which seemed to adversely criticise Dr Philip's recent researches on the ptomaines developed in the tubercular phthisical process. He thought the most natural and proper medium for the cultivation and separation of these ptomaines was the sputum itself, and he reminded the members of the Society that in Dr Philip's paper a number of control-experiments were described, in which the sputa of other diseases had been treated in an identical manner, without producing the pathogenetic derivative obtained from the true bacillary sputum. Even if it were proved that serum cultivations of the bacillus contained no ptomaine, it did not follow that the bacillus produced no such body in the living tissues and in the sputum, for it was difficult, if not impossible, to prepare an artificial medium for cultivation that should entirely represent the conditions present in the living body.

Dr Troup explained that the cultivation he showed was made on glycerine agar-agar, which Koch informed him was much better than other mediums. When he spoke of the ptomaine he had no thought of any one in particular, because he had often heard the opinion expressed that it was by the development of a ptomaine that the tubercular changes were brought about. In answer to Dr Gillespie, he stated his belief that tubercle bacilli were the cause of tubercular phthisis. In other forms, such as dust phthisis, the bacilli would not be found. With reference to the influence of the atmosphere, he said the experiments went to show that the bacilli in it were not in an active state. He had seen in the Longmore Hospital a patient dying of phthisis lying for over a year beside another patient suffering from chronic bronchitis, and yet the latter did not develop phthisis. As to the elastic tissue, he had found it in the sputum of a man suffering from phthisis, as an extension of disease from tubercular iliac glands at least a month before he found the bacillus, though he made careful and repeated examinations.

Meeting IX.—June 6, 1888.

Dr John Smith, *President, in the Chair.*

I. Exhibition of Patients.

1. *Dr George Mackay* showed a case of PRIMARY SYPHILITIC SORE ON THE UPPER EYELID. The patient was a boy, aged 6, first seen at the Eye Dispensary, Cockburn Street, on 11th May 1888. He was then found to be suffering from a small inflammatory swelling about the size of a pea, situated on the right upper lid near the inner canthus. On the external surface of the lid the skin was unbroken except in the neighbourhood of the cilia, where there was a fine, thin, semi-transparent scab. On everting the lid slightly it was found that the mucous membrane on the inner aspect of the swelling was eroded and secreting a little muco-pus. There was no abrasion of the ocular conjunctiva, and only slight injection of the conjunctival vessels. The only history that could be obtained on that occasion pointed to its existence for about a week previously as a small stye. The majority of styes pointed at the lid margin, but Dr Mackay had occasionally seen them discharge their contents by sloughing through the mucous membrane posteriorly. In such cases they probably had their origin in the Meibomian glands as opposed to the sebaceous follicles. Though his suspicions were aroused in this boy's case, so as to render him rather doubtful of its simple nature, he determined to try the effect of several small poultices and a mild antiseptic lotion. Three days later the little nodule had increased in size chiefly towards the mucous surface, and the ulcerative character appeared more decided. There was also some enlargement of the preauricular and submaxillary glands. Three sources of infection suggested themselves as possible causes of the condition:—(1.) Irritation by dirt or discharge from a non-specific source; (2.) Vaccination; (3.) Soft or hard chancre. Against the second there was the fact that no one had been vaccinated in his family lately. The swelling in the lid was moderately firm, but not of cartilaginous hardness, and, saving for the absence of infection of the opposing surfaces and the rarity of that condition in this situa-

tion, appeared more like a soft chancre than a primary syphilitic nodule. Both Dr Argyll Robertson and Mr Berry, to whom the case was shown on the following day, inclined to the opinion that it was a primary syphilitic sore, and specific treatment was immediately commenced. A papular rash, which appeared on his body a few days later (shown to the Society), confirmed that opinion, though it was rather an unusually early manifestation of the constitutional affection, having presented itself barely six weeks after the first appearance was noted in the lid. A careful inquiry into the history seemed to leave no doubt as to the specific nature of the infection, and revealed a very sad state of matters. The mother was a respectable widow, who took in students as lodgers. There was little doubt that one of these suffered from specific disease last winter. In some way or other which cannot be traced, the mother has been infected. She acknowledged to having used a toothpick of his on one occasion while suffering herself from an abrasion inside her cheek produced by a badly fitting plate of teeth. She now presented exceedingly well-marked secondary symptoms. She had extensive ulceration of the gums and mucous membrane of the lips, cheeks, tongue, and fauces, and a serpiginous eruption on the chin and side of the neck, which Dr Allan Jamieson had pronounced to be of syphilitic origin. While suffering from these symptoms she had frequently kissed the boy, and thus communicated the infection to him. The little boy had as yet no further symptoms, and the primary sore and enlarged glands were now diminishing.

2. *Professor Grainger Stewart* introduced—(*a.*) A PATIENT, B. R., 58, a married woman; has had a hard life, with many privations, but has always been temperate. She has had several illnesses, but the only one of importance was a "rheumatic fever" sixteen years ago, which lasted for six months, was unattended by sweating, and in the course of which there appeared ptosis of the right eyelid, paralysis of the right internal rectus, and motor paralysis of the left leg. After recovering from this attack she suffered from headaches, vomiting, and polyuria, and soon afterwards the attacks from which she now suffers were first observed, and they have continued ever since. Her general health is good; she passes about 100 oz. of urine daily, which, however, is normal in every respect. In regard to the nervous functions, she has frequent headaches, giddiness, and subjective sensations of heat and cold, and some paralysis, both sensory and motor, of the right leg. In both eyes there is staphyloma posticum, and some optic atrophy; the right eye is blind, and its movements are very imperfect; the left eye is very hypermetropic. Hearing and taste are normal; there has been complete anosmia for fifteen years. The reflexes are fairly normal. Her intelligence and memory are unimpaired. The interesting point in the case, however, is this,

that whenever her sound eye is closed, and under certain other conditions, she takes a fit, in which she falls forward, giving an inspiratory snort at the moment of falling, and then remains unconscious for about half-a-minute, and gradually recovers. There are no general convulsions, but the left eye is turned upwards, and remains so till the end of the fit. During unconsciousness faradic currents and other strong stimuli fail to rouse her. Various experiments have been tried, such as sudden darkening of the room while she was conversing and bringing a hood over her face, which would point to arrest of retinal stimulation as being the cause of the seizures; but, on the other hand, other experiments show that the attack comes on when the eyes are open, and light is reaching the retina if anything is placed in such a position as to cut off the view of surrounding objects. Most careful observation has satisfied other observers as well as Prof. Stewart that the symptom cannot be explained on the theory of malingering; the process is exactly the same as it was sixteen years ago. She has never had any hysterical or general convulsive attack. Beyond the existence of two cicatrices in the pharynx, and the history of the so-called rheumatic fever, Prof. Stewart had not been able to obtain evidence of syphilitic infection. (*b.*) A lad from Northumberland with acute BULBAR symptoms. He was engaged in the end of March in clearing the rails after the snowstorm. He was seized sometime after with acute bulbar symptoms, due evidently to inflammatory changes in the medulla. These were gradually disappearing. The saliva still overflowed. The movements of the tongue were impaired, and the uvula and soft palate paralyzed.

3. *Dr Brakenridge* showed a case of a lesion of the LEFT HALF OF THE CERVICAL REGION OF THE SPINAL CORD. The patient was a waiter, who had taken alcoholic stimulants for ten years past. After a week of pretty heavy drinking he woke up one morning quite paralyzed. His condition was at first taken to be one of hemiplegia from cerebral paralysis. On a careful examination he was found to have the following symptoms distinctive of unilateral spinal lesion. On the one side there was motor paralysis, which has passed off to some extent, also on the same side marked impairment of the muscular sense; also some vaso-motor disturbance, there being pallor and a slight degree of coldness; marked increase of the deep reflexes; sensibility unimpaired and the cutaneous reflexes diminished. On the opposite side there was impairment of cutaneous sensibility in all its forms, complete analgesia. The point of chief interest was the difference of the reflexes on the two sides. On the side where sensibility was impaired there were exaggerated tendon reflexes; on the side where sensibility was impaired were diminished cutaneous reflexes.

4. *Dr Allan Jamieson* showed a boy who had suffered from FAVUS, to illustrate the effect of a method of treatment which had lately come into vogue. Since the discovery of resorcin, the treatment of this affection had been much facilitated. The patient's head was shaved and poulticed, washed with black soap and warm water, and then a paste applied consisting of one drachm of resorcin to two drachms each of lanolin, vaselin, zinc oxide, and starch. Sometime after, in order to see if the result could be hastened, epilation was performed, and the same ointment applied. A curious result followed. The epidermis became loose, and could be stripped off in flakes. It had been examined and no parasite found. The hair had begun to grow in all over the scalp. The spores in this treatment disappeared before the mycelium.

II. Exhibition of Specimens.

1. *Dr P. H. Maclaren* exhibited—(*a.*) A specimen showing the spread of a MELANOTIC SARCOMA by the lymphatics. The patient was a Perthshire shepherd, aged 58, of good physique, powerful and active, and with exceptionally good personal and family histories. Till December 1886 he had not suffered from any illness. The first symptom of his trouble was then felt. When walking barefooted in the lobby of his house he imagined he had trodden upon a nail, as he perceived an unpleasant pricking sensation in his heel. Examining it he found a little elevation about the size of a grain of corn, and blackish in colour. He considered it was merely a hard corn, and did not trouble himself about it. The nodule went on growing without causing him much annoyance till the end of August 1887, when it had become so large that he felt it necessary to consult a doctor. It was then treated by the application of caustics, poultices, and ointments, but all the while steadily increased in size. A second growth also formed, which after some time coalesced with the first. In March of the present year he noticed a swelling in the groin of the affected side. He was then advised to enter the Infirmary, which he did on the 9th April. Two days before he had worked twelve hours dipping sheep, though in considerable pain. The tumour was then found to consist of a black elevated mass, occupying the under surface of the heel, and extending forwards and inwards to the inner side of the sole of the foot. Its margins were ill-defined, the odour offensive, and it was pulpy to the feel. Several black spots were visible around it in the subcutaneous tissues. These were found to contain granules of pigment. The femoral lymphatic glands formed a large mass occupying Scarpa's triangle, but not adherent to skin or vessels. The urine was normal in amount, dark in colour, and answered to the tests for indican. The limb was amputated on 19th April in the upper third by Teale's method, and the glandular mass in the groin freely and entirely removed. The glands were deeply pig-

mented and enormously enlarged. The amputation and groin wounds have both healed satisfactorily. The urine for some time after the operation gave the reactions of indican very faintly or not at all, but the reaction has again become marked. The lymphatic vessels had been dissected, and showed pigment masses continuously throughout their course. It was not often that malignancy was so clearly manifested in three distinct forms in a single case,—involvement of neighbouring textures, lymphatic absorption, and implication of the blood, as shown by the presence of melanin in the urine. (*b.*) A URETHRAL CALCULUS from the same patient. It was only discovered when the catheter had to be passed after the operation. It had been formed *in situ*, and weighed 58 grains.

2. *Dr J. M. Cotterill* showed FIFTEEN INCHES OF LARGE INTESTINE RESECTED WITH SUCCESS IN A CASE OF UMBILICAL HERNIA. The patient was an enormously stout woman, weighing 18 or 19 stones, and was seven months pregnant. An umbilical hernia of some standing came down and could not be returned. Her medical attendant put on a firm binder, and sent her into the Maternity Hospital, under the impression that the child was dead, and that she was about to miscarry. On arrival there her condition was at once recognised, and she was transferred to the Infirmary. When admitted she was collapsed and *in extremis.* There was a large bright red tumour on the top of an enormous abdomen. Dr Cotterill resolved to give her the chance of an operation. An incision 9 inches in length was made and the sac opened into. A gangrenous and burst intestine and gangrenous omentum were then found. The omentum, sac, and sloughs of cellular tissue were removed, and fifteen inches of gangrenous large intestine cut away. The two ends were left at the umbilicus, no attempt being made to unite them. The woman was got back to bed alive, and fed per rectum. Thirty-six hours after the operation she gave birth with one pain to a child, which lived for several hours. Afterwards the woman made an uninterrupted recovery. She still defæcated at the umbilicus, but Dr Cotterill hoped to remedy that by a second operation. The part of intestine removed was from the transverse colon.

3. *Dr Shaw M'Laren* exhibited—(*a.*) A specimen showing the EFFECT OF TWISTING THE SAC in the operation for the radical cure of hernia. The patient on whom he operated was not a favourable subject, being over 70, and having had a pneumonia four weeks before and a hemiplegia at the time of operation. A strangulated hernia required operation, and after it was performed the sac was twisted after the manner recommended by Mr Ball. In the specimen there was a dimple of the peritoneum at the internal ring instead of the projection which Mr Ball obtained by experiments on the cadaver. The canal was otherwise perfectly plugged by the

twisted sac. (*b.*) A SCIRRHUS OF THE MALE BREAST removed by Dr Watson along with the axillary glands.

4. *Professor Grainger Stewart* showed a specimen of SULPHONAL, a new hypnotic, which had been made known to the profession mainly by Professor Kast of Freiburg, and which had been used with much success by Prof. Stewart. Doses of 15 grains often produced satisfactory sleep, and doses of 30 or 45 rarely failed to do so. No injurious effects had been observed to follow upon its use, and its taste and smell were not unpleasant.

III. Original Communication.

TWO EPIDEMICS OF SORE THROAT AND THEIR RELATION TO THE MILK SUPPLY.

Dr J. M. Cotterill read a paper, of which the following is an abstract, giving the history of two epidemics of sore throat occurring at Fettes College. The first of these epidemics began in October 1886, and comprised 50 cases; the second appeared exactly a year later, and comprised 84 cases. Both began about three weeks after the assembling of the boys after the summer vacation, and both commenced alike by several boys being taken ill on the same day, these boys coming from different school-houses, and most of them not having been in any close contact with each other.

The symptoms of the disease were as follows: When first seen the patient was generally out of sorts, complaining of headache, want of appetite, and lassitude; occasionally there was sickness or nose-bleeding; in most cases a furred tongue, foul breath, and other symptoms of gastric disturbance. The tonsils and posterior wall of the pharynx were bright red, with considerable swelling of the mucous membrane. The uvula and soft palate were also congested, but in no case (out of 130) was there any membranous deposit on these parts. Upon the tonsils and wall of the pharynx, however, there were always follicular exudations, and sometimes a considerable patch of deposit on the tonsils, which were often much swollen and deeply fissured. These patches could always be easily removed. Albuminuria was very infrequent. The disease usually lasted about seven days in its acute stage. A peculiar symptom was the implication of the uppermost of the chain of lymphatic glands behind the sterno-mastoid. This always happened several days after convalescence had commenced. There would be a large brawny swelling in the neck, which in every case subsided slowly without suppuration. Rheumatic pains in the neck and back were frequently noticed during convalescence, and in three cases rheumatic fever followed the sore throat.

In the case of the first epidemic, Dr Cotterill had the drains of the College dairy examined, and as slight defects were found, the disease was thought to be due to this cause. While these defects were being remedied, the milk given to the boys was boiled, and four days after this practice began the epidemic stopped suddenly, and did not appear again for some three weeks. Then, upon the milk being given unboiled, the cases began again. The epidemic, however, gradually died away, 50 cases in all having occurred, of which all but five or six appeared before the milk was boiled, or within four days after that practice began. There was not a single case of similar sore throat in the school till the very same day in October 1887 upon which the epidemic had begun in 1886.

At the commencement of the second epidemic Dr Cotterill naturally suspected the dairy drains again to be at fault; but being assured by the sanitary officers that they were in perfect order, he was doubtful as to whether the milk was really the medium of infection. To test this he again ordered the milk to be boiled. By this time the epidemic had lasted eleven days, and some 50 cases had already appeared. Four days after the boiling of the milk began the epidemic suddenly stopped. Meanwhile the cows had been inspected by Professor Williams, and two of them had been certified to be suffering from "variola vaccinia" in its later stages. These cows were separated, and no further sore throats occurred for thirteen days. The attendants at the dairy had also been carefully examined by their ordinary medical attendant, but no present nor recent illness could be discovered amongst them to account for the epidemic.

To test the value of the separation of the diseased cows, the boiling of the milk was then discontinued (*i.e.*, after no sore throats had appeared for thirteen days). Three days later the epidemic began again. Dr Cotterill then asked Dr Woodhead to visit the dairy with him, and they found two new cases of vesicular disease in the cows which had not been separated. On this occasion the owner of the cows, to prove that there was no connexion between the cow disease and the sore throats, pointed to, as she believed, a healthy cow, the milk of which had been reserved for a baby of one of the customers, this baby, notwithstanding, having contracted the sore throat. To the dismay of the owner, however, it was discovered that this cow had a well-marked vesicular eruption on the inner aspect of one of her teats. After separating these two cows the epidemic again stopped for five days, but then new cases began to appear again. This was possibly due to the fact that, to make up for the diminution of the supply from the separation of the diseased cows, milk, admittedly of an inferior quality, had been got from an outside dairy and mixed with that going to the College. An arrangement was accordingly made for supplying two of the schoolhouses from another dairy altogether, while the rest of the College was supplied with milk from the healthy cows of the College dairy. After that, though the milk was no longer boiled, the epidemic ceased altogether.

Dr Cotterill had heard of at least nine houses in the west end of the town where these sore throats had attacked several members of the family, and in every single instance the milk was supplied from this College dairy. He was not able to convince himself that this sore throat was contagious, for when the milk was boiled affected boys were going about freely among their fellows, but did not seem to disseminate the disease, nor did the disease affect groups of boys who were specially thrown in contact with one another. Out of the fifty boys attacked in 1886, only one suffered in 1887. The total percentage of boys attacked in the second epidemic was about 35 per cent.

The chief points which appeared from a consideration of the foregoing facts were as follows:—1. That this form of sore throat, while presenting certain points of resemblance to follicular pharyngitis and to diphtheria, was absolutely distinct from both, and was a form of disease not usually recognised nor described. 2. That it was due to the milk supply. 3. That it was possibly due to some form of vesicular disease in the teat of the cow. 4. That an almost absolute immunity from a second attack was obtained by a first attack. While these propositions were not brought forward as absolutely proved facts, Dr Cotterill thought the evidence strong in their behalf. The milk supply had lately played the part of scapegoat, and while it was most inexpedient to cause unnecessary alarm, it was imperative that such facts should be looked in the face.

A paper by Dr G. S. Woodhead relating to an inquiry into the bacteriological examination of the boys' throats, matter from the diseased cows' vesicles, etc., was read; but as the inquiry is still incomplete, fuller particulars will be given at a later date. At present it is sufficient to say that several micro-organisms common to the sore throats in the boys and the milk and vesicles in the cows were discovered.

Dr M'Fadyean said that he had listened with great pleasure to the paper which had just been read by Dr Cotterill, but he must say that this pleasure was alloyed with a slight degree of apprehension lest Dr Cotterill might be able to prove that the epidemic of sore throat was due to the milk. Certain facts, however, within his own knowledge tended to make that apprehension very slight. He (Dr M'Fadyean) had had under observation the dairy to which reference had been made almost from the very outset of the sore throat epidemic. He thought he would be able to state some facts which, if they did not prove that the outbreak was not due to the milk supply, would at least go to show that it was not in any way connected with the cow disease. His own connexion with the dairy was as follows:—He ascertained from Professor Walley, of the Royal Veterinary College, about the middle of October last, that there was an outbreak of eruptive disease on the udders and teats of the cows at this Comely Bank dairy. At that time he was conducting a series of experiments for the Agricultural Department of the Privy Council, and in one of his reports to Professor Brown he mentioned this epidemic. He received a reply requesting him to keep the disease under observation, and to spare no expense that might be considered necessary to elucidate the nature of the disease. The consequence was that he had the dairy in question under close observation from the 22nd of October until the middle of January. Now, unless there was proved a clear correspondence between the course of the outbreak in the cows of this dairy and that of the sore throat epidemic, Dr Cotterill's contention that the cow disease was the cause of the sore throats would entirely fall to the ground. He thought he could show that there was no connexion in point of time to be traced between the incidence of the cases in the cows and the course of the epidemic in Fettes College; he thought, in fact, he could show that the disease existed in the dairy long after the sore throat epidemic had ceased. The facts that to his mind would demand from Dr Cotterill an explanation were as follows:—There were no cases (if he had noted correctly what Dr Cotterill said) from 22nd October till the 7th of November. Dr Cotterill ascribed this change to the separation of the affected cows.

Dr Cotterill.—No, to the boiling of the milk as well as to the separation of the cows.

Dr M'Fadyean.—What was the last date upon which the milk was boiled?

Dr Cotterill.—The 5th of November.

Dr M'Fadyean (continuing) said that from the date of his first visit on the 22nd of October until the middle of January he saw continuously in the dairy cows affected with this eruption. From the 10th to the 22nd October, there were four cows attacked with this vesicular eruption, and six from the 24th October to the 12th November. There were new cases on each of the following days:—on the 18th, 19th, and 30th November; 9th, 11th, 21st, and 24th December; on the 5th January, and about the middle of January. There could be no question whatever that these cases were all of the same character. That led him to refer to the inquiry as to what was the character of the eruption on the udder. Dr Cotterill incidentally mentioned in his paper that Professor Williams had in his report stated that it was cow-pox. He was very much surprised that any one with the experience of Professor Williams should have been led to give that opinion, because to his mind there was not one of the features characteristic of cow-pox present in this eruption.

Dr Haddon.—Did Dr M'Fadyean see the cows referred to by Professor Williams?

Dr M'Fadyean replied that he had—that he had seen every case that occurred in the byre from the beginning of the outbreak till the middle of January. He had been able to watch the complete course of the disease, which he had described in a report recently furnished by him to Professor Brown. Besides he had had drawings made showing the eruption in its various stages. The eruption in these cows had the character of unilocular vesicles rapidly becoming purulent, there was no areola, and there was no subsequent pitting. The latter feature was perhaps not distinctive, but to settle the matter he had made the following experiment. A calf was inoculated with matter from a recent vesicle on one of these cows. As a result a slowly healing sore formed at the point of inoculation. After a month this calf was again inoculated, this time with vaccine (calf) lymph, and the inoculation took in a typical manner. He had been careful to perform this experiment in anticipation of any one concluding that the disease had been true cow-pox. He was distinctly of opinion that it had not been proved that the epidemic was caused by the milk supply through the cows having been affected with this vesicular disease of the teats. The milk in this case was sent immediately after milking to Fettes College, and he asked was it not just as possible that it became contaminated after it reached the College? He did not think it necessary to refer at length to Dr Woodhead's part of the inquiry, for as Dr Woodhead himself pointed out, the results where reliable were negative. He entirely concurred in Dr Cotterill's opinion that, while we should not jump to hasty conclusions, the facts where they pointed to milk contamination must be faced. He had taken up the inquiry with his mind quite open to find that there was a causal connexion between the cow disease and the sore throat epidemic, but the facts forced him to an opposite conclusion. He did not feel justified in denying that the milk supply was the cause of the epidemic, but he certainly thought that Dr Cotterill was not justified in concluding that "the epidemic was due to the ingestion of milk from cows that were suffering from one of the many forms of vesicular disease of the teats," if by that he meant that the vesicular disease was the cause. In conclusion, he asked Dr Cotterill to explain how it was that out of the 65 per cent. of pupils that remained open to infection after the 20th November not one had been attacked, although to his positive knowledge milk taken from affected cows had been regularly sent to the College up till the middle of January. He did not think Dr Cotterill could explain that—he hoped he could not—and he was therefore happy to think that Dr Cotterill had not added a new terror to the consumption of milk, that in fact he had not established the existence of another disease which could in this way be transmitted from the lower animals to man.

Dr Foulis considered that the epidemic described was caused by the milk supply. It did not matter whether it was from the vesicular disease of the teats or not. The evidence that the milk supply was at fault was to his mind convincing. It was therefore of importance that such dangers in the milk supply should be checked at once, and the only way of doing this was by boiling the milk. Dr Foulis further referred to the scarlet fever epidemic of last year, and read a letter he had from Dr Russell of Glasgow regarding an epidemic in that city. In both cases the epidemic was undoubtedly caused by contaminated milk. Nothing but the most stringent supervision of all dairies and farms which supplied milk, and the thorough boiling of milk, just as we cook all other animal foods, would prevent these dreadful epidemics.

The President reminded the members that the question at issue was whether the vesicular disease on the teats of the cows was the cause of the sore throat epidemic or not.

Dr Peel Ritchie said there were two points on which he should have liked more information. The first was, whether the patients had any spots under the tongue or elsewhere in the buccal cavity except on the fauces; and the second was, whether the people engaged in the work of the dairy had been examined to see if any eruption appeared on any part of their person, such as the hands. He did not think Dr Cotterill had brought before them a new disease. His recollection went back to 1863, when Dr George Balfour brought before them two cases recorded by Dr Hislop of Renfrew, in which two persons were affected with vesicular eruption, and others with sore throat, after drinking the milk of animals suffering from epizootic aphtha or murrain. When he wrote his paper on *Stomatitis* he drew attention to similar instances recorded in the *Veterinarian*. It seemed to him that the vesicles described were nothing more nor less than a form of murrain. As for the epidemic, he could not but believe that it resulted from the milk supply.

Dr Brakenridge said he was one of the physicians who was in attendance on four families beyond the Dean Bridge, the members of which suffered from symptoms corresponding exactly with those described by Dr Cotterill. In one of the families the swelling of the glands was a very marked feature. He was puzzled to know the cause of the outbreak, and had the house drains overhauled without finding anything conclusive. It was not till the lady of the house informed him of a remark innocently made by the dairyman to the servant, who told him of the illnesses in the house, when he said that all the houses he was going to seemed to have the same trouble. He did not know of any case occurring in which the milk was not supplied by this dairy. The point whether the vesicular disease of the cows' teats was the cause of the epidemic was not so well made out. The thing that struck him was the isolation of the disease, and the confinement of it to one dairy, and it raised the question whether there might not be some other condition present in the dairy which had not yet been found out.

Dr Hare said the dilemma in which they were placed with regard to this question was a very complete one. He ventured, however, to suggest that there was a possible way out of it. It seemed to him that there must be something more than the mere occurrence of a special disease in animals before it passed to man. In connexion with this they must look for further aid to the veterinary and bacteriological sides. The clinical work had been completely carried out. It might be that disease in animals could be conveyed to man only at certain stages, or at certain periods of the year.

Dr M'Kenzie Johnston thought the epidemic was not so local as Drs Cotterill and Brakenridge took for granted. He had seen several cases, apparently exactly similar, where there was no cause to suspect the milk, and which was from a different source. Dr Cotterill, he thought, had proved in his own cases that the milk was the carrier of infection, but Mr M'Fadyean had gone far to disprove the connexion of the epidemic with this vesicular cow disease. He was inclined to think that germs from decomposing organic matter—however introduced into the body—were a likely cause of sore throat. Might not the cessation of the epidemic be due to increased cleanliness and precautions taken by the proprietor on finding his byre constantly under supervision?

Dr Russell of Davidson's Mains asked if Dr Cotterill had taken into account a burn running past Fettes, which was in a very filthy condition, and had to be cleaned out shortly before the epidemic occurred.

Dr Burn Murdoch had seen one or two epidemics of sore throat, which had completely puzzled him. In one instance sixteen cases, with characters not unlike those described by Dr Cotterill, occurred within a week. He took the trouble to investigate the milk supply, and found that all of them got their milk from one dairy, near to the Barclay Church, which got its milk from Davidson's Mains.

Dr Allan Jamieson said that during the time when this epidemic was going on he had carefully watched all the cases in the City Hospital to see whether any symptoms occurred like those which characterized this epidemic, but he saw none. The glandular swellings described did not appear in any of their cases. This favoured the views of Drs Cotterill and Brakenridge that the disease was one *sui generis*.

Dr Cotterill, replying, said he must congratulate himself that the clinical opinion was on his side that this was a new disease, or at anyrate one not generally recognised. While he sympathized with Dr M'Fadyean in not wishing to make a further scapegoat of milk, he could not help believing that the milk supply was at the bottom of the epidemic. Whether it was the particular vesicular disease of the teats which was the cause was a point on which he was rather half-hearted. That it was the milk he was certain, but what condition in the milk he could not say. An interesting question which he should like to put to Dr M'Fadyean was whether the vesicles were distinguishable. He had seen it stated that there were seven or eight forms of vesicles which affected the teats of cows, and that it was frequently impossible to distinguish these.

Dr M'Fadyean said he could now distinguish this particular vesicle.

Dr Peel Ritchie asked if the condition was one of murrain.

Dr M'Fadyean.—No.

Dr Cotterill, resuming, said the fact that the epidemic in the dairy went on after that in the school ceased did not prove that the epidemic in the school was not due to that in the dairy; for not only were fifty boys protected from the second epidemic by having suffered the year before, but also they were quite ignorant of the special conditions which caused such a disease in the cow to be harmful at one time and not at another. The boys had no spots or aphthous patches anywhere. The dairy people had all been carefully examined, and none of them had sore throats or sores anywhere either at the time of the epidemic or shortly before it. Dr Mackenzie Johnston thought he had seen similar cases. Probably that was so, and probably there might be cows in other dairies affected in a similar way. As to the burn, it did not run through any part of Fettes where the boys could come in contact with it. It had been suggested that the cows might have fed on contaminated grass along its banks, but they were unable to get any proof of that. Dr Burn Murdoch's observation was very important. It was possible that the burn might have been the cause of the epidemic he (Dr Burn Murdoch) observed, seeing that it passed through Davidson's Mains, from which the suspected milk came. He was disappointed that no reference was made in the course of the debate to the improper use of the term "diphtheritic" in connexion with such cases.

Meeting X.—July 4, 1888.

Dr Clouston, *Vice-President, in the Chair.*

I. Exhibition of Patient.

Dr Allan Jamieson exhibited a patient exemplifying the condition described by Dr Walter G. Smith of Dublin (*British Medical Journal*, 1st May 1880) as nodose hairs. He was a well-grown boy aged four years. The scalp was fairly well covered with dark hair, which, however, in no part exceeded half an inch in length, in most parts was still shorter. It felt to the hand passed over it harsh and wiry. On examining the hairs under the microscope, most of them showed more or less distinctly a regular alternation of swellings and contractions, exactly similar to those figured by Dr Smith. The nodes were pigmented; the contracted portions were devoid of colour. Scarcely any trace of imbrication was visible on the nodes, but could be plainly made out on the contractions. By staining the hair with nitrate of silver, and shaving after an interval of four days, it was found that each node took two days to grow, which also corresponded to the estimate of Dr Smith. From an examination of the hairs, which were extracted with the root and the root-sheath *in situ*, it was seen that the contraction and node formation did not occur within the follicle, but first appeared on the hair-shaft a short distance beyond its point of exit from the follicle. Possibly, therefore, the condition is due to a rhythmical imperfection in the cuticle of the hair. Where this is well pronounced the hair is narrowed, its fibrous structure being, as it were, forced out to form the node. At all events the change in form is a secondary one, and takes place when the hair becomes subjected to the desiccating influence of the air. Two other children are similarly affected,—one a boy aged $6\frac{1}{2}$, the other a girl, aged 2. In neither does it assume such well-marked proportions, as it only implicates the occipital region, the hair growing pretty well in front, though in the boy it is too freely shed. No history of any similar defect in an ancestor can be made out. The father is alive and well. The mother died at the age of 30 of cancer of the uterus.

II. Exhibition of Apparatus.

1. *Dr A. Bruce* showed Leiter's ENDOSCOPE, and demonstrated its working in an artificial bladder.

2. *Mr F. M. Caird* showed R. W. Parker's TRACHEOTOMY TUBE with the greater portion of the upper wall of the inner canula removed. This rendered the tube more easily removable than the ordinary form, and facilitated the cleansing of the outer canula.

3. *Dr Felkin* showed Stern's GEHEIM-CAMERA, which is an instantaneous photographic apparatus. It can be carried underneath the coat with the lens projecting through a button-hole. By pulling a string the photograph is immediately taken. Six photographs are taken upon one slide, and measure an inch and a half in diameter. They can subsequently be enlarged to six or eight inches. The cost of the apparatus is only thirty shillings. It is exceedingly easy to manipulate, and can be used either to catch the fleeting expressions of a patient, or to obtain an accurate photograph of any injury. The apparatus has only two drawbacks—the one is that it requires a strong light, as the process is so rapid, and,

secondly, all the six photographs on the one plate must be taken with almost the same intensity of light, or else they develop unequally.

III. Exhibition of Specimens.

1. *Dr Byrom Bramwell* showed a sample of MILK-LIKE URINE which was not chylous, and had a very fœtid odour.

2. *Mr A. G. Miller* showed a drawing of the result of an AMPUTATION OF THE PENIS FOR EPITHELIOMA. He had saved as much skin as he possibly could, cutting the corpora considerably further back, and had thus left a fair-sized and presentable organ.

IV. Original Communications.

1. METHODS OF TREATING NASAL AND NASO-PHARYNGEAL POLYPI.

By P. M'Bride, M.D., F.R.C.P. Ed., F.R.S.E., Surgeon to the Ear and Throat Department, Edinburgh Royal Infirmary; Lecturer on Diseases of the Ear and Throat, Edinburgh School of Medicine.

To avoid misconception, it must be here stated that I propose to confine my remarks entirely to operations without external incision, and to discuss only the treatment of polypi proper, *i.e.*, mucous polypi and pedunculated naso-pharyngeal tumours. These limits manifestly exclude the formidable cases known as fibrous polypi of the naso-pharynx, and equally malignant tumours of the nose. Neither do I wish my remarks to be understood to apply to papillomatous excrescences as they occur on the inferior turbinated body.

Mucous Polypi of the Nose presenting anteriorly.

When these tumours have reached any appreciable size, it is manifestly desirable to remove them, hence we may at once dismiss treatment by interstitial injections and caustics. If from any reason this be impracticable, then such methods may be considered, and I have little doubt that the destruction of even large growths may be safely, if slowly, accomplished by means of chromic acid used in the way to be described when the treatment of small growths and the pedicles of larger ones is described.

The removal of mucous polypi is easy or difficult according as the tumours are accessible or inaccessible, but to discuss the various manœuvres required in different classes of cases would be but to repeat what is found in any standard special treatise. My main object is to compare the various operative procedures now adopted, with a view to elucidating their respective advantages and disadvantages. These methods are evulsion by means of forceps and removal by snares, which may be cold, or heated by the electric cautery.

(1.) *Forceps.*—This method has, I believe, no advantages to recommend it. If practised without illumination, it is, unless for exceptional cases, unscientific, and I would almost say unjustifiable. If, on the other hand, the forceps be guided by the eye, their use is still open to the objection that more pain is caused by opening the blades than by the introduction of a delicate wire snare. It is true that forceps sometimes prove serviceable in those cases in which the polypus, although seen by means of the nasal speculum, cannot be enclosed in the loop of the snare; but such cases are certainly uncommon.

(2.) *Snares.*—No doubt all specialists are now agreed that the snare is the best weapon wherewith to attack nasal polypi. Further, there is a general consensus of opinion that the loop should be composed of piano wire, which, owing to its resilience, can be much more conveniently used within the nasal cavities than any of the more flexible varieties. Here, however, the unanimity ends.

The advantages claimed for the electric snare, used at a red heat, are that it is—(1), not very painful; (2), unaccompanied by bleeding; (3), a caustic. All these advantages I am willing to admit, and I am quite of opinion that it is the best method of removing those polypi which are not distinctly pedunculated—in other words, fringes of polypoid tissue attached along the whole length of the middle turbinated bone. This form of nasal polypus is, however, in my experience less common than the distinctly pyriform pedunculated variety. Even in the case of these, however, I should prefer to use the galvano-caustic snare rather than a cold wire loop did I look upon the latter simply as a means of abscission. No doubt if we can always make sure that our snare is adjusted to the very base of the tumour, then the theory of the wire heated by electricity becomes beautiful beyond conception, owing to the simultaneous removal of the growth and cauterization of the base. It is, however, almost with a pang that we turn from this beautiful ideal to the experience of practice. As a matter of fact, let us set our snare ever so wisely, the actual base or point of attachment is almost never reached in such a way as to enable the operator to cauterize the point of junction between healthy mucous membrane and polypoid tissue. (I purposely avoid the word myxomatous.) If this contention be admitted, the statement that the electric snare acts at once as a means of removal, and at the same time cauterizes the pedicle, may be set aside as unsatisfactory, for obviously cauterization of the pedicle is useless, unless we are by its means enabled to destroy the point of junction of the growth with the healthy mucous membrane. This I maintain cannot often be done, although the attempt may under certain circumstances prove successful.

The argument as to the avoidance of hæmorrhage has certainly more in its favour. There is no doubt that if the electric snare be used at a dull red heat bleeding is avoided, and thus less delay is caused to the operator; and if the latter be hurried or of an impatient frame of mind, he is enabled to proceed with greater comfort. Perhaps, too, we may admit that from this point of view the heated snare is advantageous in cases where it is essential to clear the nose at one sitting. Whether such a proceeding is best for the patient I feel justified in doubting; for even the advocates of the electric snare admit that repeated séances are desirable,—nay, even necessary, if recurrence or the rapid enlargement of previously existing minute excrescences is to be avoided. Speaking generally, then, we may consider the avoidance of hæmorrhage a convenience, but nothing more.

Turning now to the asserted greater painlessness claimed for the heated wire as compared with other methods, I take the liberty of denying this directly. My own experience has been that patients suffer discomfort during the removal of polypi by the cold snare, not so much while the growth is being taken away as during the adjustment of the loop, and that the pain is directly proportionate to the endeavours of the surgeon to push the wire up to the point of attachment. After this, if the cold snare be used, the actual removal of the growth takes place so quickly that it is hardly felt. With the heated wire it must of necessity be different if the operation is to be bloodless—one of the chief merits claimed by its advocates—and if, further, anything like thorough cauterization of the pedicle is aimed at. I have myself given the method of removing polypi by means of the galvano-caustic loop a fair trial, and have been convinced by the experience so gained that although not extremely painful, it is much more so than the cold snare. This statement is based upon the opinions of a number of intelligent patients on whom both methods have been used by me.

So far I have stated my objections to the galvano-caustic loop for the removal of nasal polypi. In some cases, however, I have found it serviceable. Roughly speaking, its use seems to me indicated in cases where the sessile nature of the growth renders it impossible to practise the method I adopt in using the cold snare. I thus prefer it for the removal of polypoid fringes, because after the growth has been encircled and the wire slightly tightened, a firm hold may be taken of the tumour by turning on the current and so producing a furrow at its base. This, of course, effectually prevents the wire from slipping. However, in these and allied cases I cut off the part grasped in the ordinary way.

In the common form of pedunculated polypus, however, I am strongly in favour of the cold wire snare—used not for abscission but evulsion. I need not here enter into the question of the etiology of nasal polypi, a matter still involved in mystery. That gravitation has, however, a share in their production I believe few will deny. In most cases they are pear-shaped, and the point of attachment to the mucosa proper corresponds to the thinnest part of the pedicle, or nearly so. I believe, therefore, that if the polypus be grasped firmly and traction made upon it, this is the point at which it will yield.

No doubt the nostrils can be freed from polypi either by the cold or electric snare, and also by means of the forceps used by the aid of sight; but in this case the operation is associated with unnecessary pain, and no doubt the desideratum of the good surgeon is to accomplish his object with as little discomfort to the patient as possible.

When all that can be done by these methods has been accomplished, however, the treatment is but half accomplished. If we desire to avoid recurrence we must resort to some after-treatment. Sometimes the spray of rectified spirit, as suggested by Mr Miller, is a most efficient remedy. My only objection to it is that sometimes it causes irritation, and I am not quite certain that its continued use might not injure the sense of smell. I confess, however, that I formerly used it a great deal, and that notwithstanding this fact my fear is based purely upon theory and analogy. I now generally, as do all specialists, destroy all the blue polypoid tissue which is left, after so much as possible has been removed, by one of the above-mentioned methods, either with the galvano-caustic burner or with chromic acid fused on a roughened probe. Here again I believe it is immaterial which method is used. When chromic acid is employed a solution of bicarbonate of sodium should always be at hand for purposes of neutralization if pain be excessive. It goes without saying that in all operative procedures cocaine should be employed. In some cases a combination of cocaine and menthol seems to act more efficiently than either drug separately.

As I stated in the beginning of this paper, I do not desire to read a complete treatise on the treatment of nasal polypi, nor do I intend to refer at any length to diagnosis. There is, however, one form of polypoid hypertrophy which is apt to be overlooked, and in which the nose, although quite free for respiratory purposes, is unable to detect odours. In such cases the anosmia may be due to a polypoid fringe, running along the olfactory cleft between the septum and middle turbinated body, and of course attached to the latter.

I shall now turn to the second clinical group of such nasal polypi as come within the scope of this paper, viz.:—

Post-nasal Polypi.

Sometimes nasal polypi, growing usually from the posterior extremity of the middle turbinated body, project into the naso-pharynx, and can be seen by means of the rhinoscopic mirror resting on the palate. This occurs usually, I think, when the anterior nares are either naturally narrow or occluded by other growths.

Another form of post-nasal polypus grows from the edges of the choanae, and its histological structure is midway between the soft fibroma or mucous polypus and the true fibrous tumour. An important clinical feature about these growths is that they are usually single, and do not tend to recur after removal. In my own experience, too, they are generally furrowed, owing, I take it, to the fact that they have a long pedicle, and that the respiratory movements constantly drive them against the septum of the nose.

If the nostrils be wide, and the growth can be seen through the anterior nares, such polypi may be snared from in front by the aid of the speculum. If, however, the nostrils be narrow, this is not easy. In such cases an attempt to seize the tumour by means of a Jarvis' nasal snare with a bent tube, under guidance of the rhinoscopic mirror, is often successful. The parts must first be made insensitive by means of cocaine, which should also be applied by means of a bent brush to the posterior nares. The mirror being held in the left hand, and a strong light thrown into the throat, the wire is guided over the tumour and tightened. Traction is made whenever the growth is firmly grasped, and the result is that it usually comes away at the point of attachment. I believe that in these cases evulsion by means of the cold snare or forceps is infinitely superior to abscission by means of the cautery. Sometimes such tumours project below the palate, and they can then be grasped without using the mirror. The two specimens now handed round illustrate the advantages of evulsion over abscission. In both cases I passed the snare up as high as possible, and yet only succeeded in encircling the substance of the growth; had I used abscission a large portion would have been left; but, as it was, the whole growth with its pedicle was completely removed in each instance.

No doubt cases occur where these methods are unsuccessful. An attempt should then be made to slip a loop of wire through the nostril, guide it over the tumour with the finger, and finally thread it on to an ecraseur, as was done in the case from which this specimen was obtained. If this fails, the growth can sometimes be detached by the finger, in which way the example now shown was obtained. The most difficult case I have had to deal with lately was a mucous polypus resting on the palate, which could not be reached by means of snare or forceps. The method adopted was to plug the corresponding nostril with a tightly fitting piece of lint. Thus the growth was drawn forward, and caught by means of catch forceps, over which a snare was slipped.

Fringes of polypoid tissue situated far back in a narrow nostril are often extremely difficult to remove, and impossible to thoroughly eradicate without an external operation, the grave nature of which naturally deters one from suggesting it for a comparatively trivial ailment. No doubt in these cases forceps can be used, only the whole base of the polypoid tissue can rarely be eradicated in this way. Then, again, bent burners may be introduced behind the palate, but it is difficult thus to reach any but the hindmost growths. I have not, so far, used the blunt hook, as recently recommended by Lange, who, however, advocates it more for pedunculated than sessile growths.

To meet such cases I have had made what I may term a galvano-caustic nasal plough. Although I have used it successfully in hypertrophies of mucous membrane, I have only so far employed it once for sessile polypi, and this case is still under treatment.

The after-treatment of the naso-pharyngeal tumours under discussion varies. In the fibro-mucous variety no further proceedings are necessary if the extirpation has been complete. Mucous polypi must, however, be dealt with in a manner exactly similar to that already described in connexion with those which present anteriorly. This is not so difficult as might be expected, as in many of these cases the pedicle attached to the posterior extremity of the middle turbinated body is distinctly visible from the front, if only a suitable speculum and good light be employed.

Mr Duncan thought Dr M'Bride had been a little severe on the forceps. He did not think the difference between the snare and the forceps was of much moment. If he gave the preference to either it would be to the forceps, because he could take away more polypi with it than with the snare. The time occupied was also greater with the snare. The amount of pain given with the one or with the other was really a question of delicacy of manipulation. He also found that he could remove those polypi which projected posteriorly with the forceps more easily than with the snare. It not unfrequently happened that it was impossible to illuminate the parts sufficiently to be able to put on the snare, but with the finger in the pharynx the forceps could be properly guided to the spot. Of the solitary polypi to which reference had been made he had met with three or four examples. They tended to occur in young people, projected to the back, were somewhat firmer than the ordinary mucous polypus, extremely pedunculated, and easily separated. They differed greatly from the fibrous polypus which grew from bone, and appeared to be analogous to the pedunculated fibrous polypus of the rectum.

Dr M'Bride said he did not condemn the forceps, but the blind use of it which was so common, and which he did not think Mr

Duncan would defend. As to the question of pain, he drew his conclusions from the statements of intelligent patients who declared the snare to be much less painful than the forceps. For impatient patients the forceps was certainly quicker, but he doubted if it were better for them, as usually no attempt was made to prevent the recurrence of the growths. He was inclined to look upon the solitary polypi referred to as midway in structure between the ordinary mucous and the fibrous growths, which he had described in his paper as the fibro-mucous variety.

2. TRACHEOTOMY IN CHILDREN, WHY UNSUCCESSFUL?

By Alexander Thom, M.D., C.M., Crieff.

Seven times in the course of my work as a general practitioner have I performed the operation of tracheotomy, as a last resource, for obstruction of the larynx due to disease in children, and although the immediate result was entirely satisfactory, in every single instance the disease has proved fatal. I am well aware that it is not so usual for a member to bring before this Society a series of unsuccessful cases as the reverse, and perhaps I ought to apologise to the Society for venturing to bring before it a subject like tracheotomy, which in the eyes of the city members, at least, has possibly been discussed already *ad nauseam*. I find, however, that the question of tracheotomy in croup (to which disease my remarks and cases more particularly refer) is by no means a settled or defined one among general practitioners; and I find that in performing that operation, I am at variance with the practice of many excellent and experienced provincial general practitioners. Moreover, the earlier treatment of croup seems to me eminently unsatisfactory; and if I can elicit from the members any method of treatment which has been found really serviceable, and which is not to be found in our ordinary text-books and monographs, I feel that I may be pardoned for taking up your time. It would certainly be of infinite value to general practitioners, if we could relegate to tracheotomy its true place in the treatment of laryngeal obstruction in children from acute disease. A mere croupy cough from slight laryngitis is often called croup, and as often easily cured; but true membranous croup seems to me a very terrible and dire disease, and, indeed, my personal experience makes me stand more in awe of it than of diphtheria even, unless the latter is in an advanced stage and in an inaccessible region.

Moreover, the extraordinary interest manifested in the case of the late lamented Emperor of Germany has made the public so conversant with the operation of tracheotomy, that in order to hold our own in ordinary conversation on the subject, it is very necessary that we should have clear and well-defined views, and consequently the present appears to me a fit time for the examination of such.

Further, I have not forgot a remark made by a city member, when speaking to me privately regarding one of the papers by Dr Hunter of Linlithgow. It was to the effect that country members conferred on the Society a real service by bringing before it their difficulties. I am, therefore, emboldened to lay before you a real difficulty, but am very far from claiming for this short communication any such merit as was justly awarded to Dr Hunter's excellent papers.

In the following series of seven cases which extend over a period of ten years, I shall as briefly as possible mention the course of the disease before and after the performance of the operation.

Case I. — A girl, aged 3 years, the patient of another practitioner—had been suffering from croupy cough and difficulty of breathing, with feverishness, for about a week, and had been under medical treatment during that time. Happening to live near I was called in. Finding the child almost pulseless and making only a few gasping efforts to breathe, and learning that croup was the cause of it, I lost no time in opening the trachea. This was followed so far by relief; the child made one or two attempts to breathe and the pulse flickered, but as speedily sank again, and she died quietly in spite of artificial respiration persevered with for a considerable time.

Case II.—A girl, aged 2½ years, had been attended for several days by my late father for croup and bronchitis. In spite of the usual remedies the breathing got more and more embarrassed, there being great falling in of the supra-sternal notch and abdominal parietes and lower ribs on inspiration. Orders were left that we were to be summoned if any more marked difficulty of respiration occurred. This happened about four o'clock in the afternoon, when I opened the trachea in the usual way. The breathing immediately became almost natural, though cough from the bronchitis was rather troublesome. We rigged up a blanket tent, and kept the air moist by putting red hot bricks at intervals into a tub of water. Dr Hector M'Lean Wilson (now of Penrith, but then a medical student) kindly stayed most of the night beside her, and saw to the continuance of the treatment, and the child was extremely well next day. Towards evening, however, symptoms of lung congestion supervened, and the child died quietly thirty-six hours after the operation.

Care III.—A lad of 9 years also occurred in the practice of my father, who had been attending him for some days for true croup. Symptoms of acute obstruction having supervened, I was called in late at night, with the object of giving mechanical relief if possible, the parents having at last given their consent. I at once performed tracheotomy with the usual precautions, and though all the light I had was from an ordinary candle, the operation was perfectly successful. Membrane and mucus were coughed up, and the tube having been inserted, the patient took nourishment readily and with relish, nodded and shook his head, and otherwise made signs in a perfectly natural manner. Next day his temperature was almost normal, and except for the fact that he was breathing through a tube he seemed perfectly well. The air was kept well moistened as before, and I felt almost certain that the case would end satisfactorily. In the evening, however, I was sent for in a hurry, and found him suffering from acute abdominal pain, which so far as could be made out, he localized about the right iliac fossa, but he was writhing and twisting about so much that nothing definite could be made out. His breathing was perfectly good. He was ordered a full dose of opium or morphia, which was to be repeated in an hour and a half if necessary. When I returned from the country about midnight, I found that he had just died breathing easily to the end, but suffering severe abdominal pain. Next day we made a post-mortem examination, and found a simply perfect example of recent invagination of the bowel, about 9 inches of the jejunum being invaginated. There was no obstruction whatever below the tube, and no extension of the membrane into the trachea.

Case IV.—A lad, 10 years of age, living at a distance of eight miles, had been seen by myself for three days before any symptoms occurred of laryngeal obstruction. He had a slight cough, general malaise, and an ulcerated throat, with some sloughy patches which were suspiciously like diphtheria, and which I treated as such. They speedily disappeared, and the throat improved, but a croupy cough came on, and soon the breathing was embarrassed and difficult, and the usual signs of laryngeal obstruction supervened. Having explained to the parents what should be done as a last resource in the event of threatened suffocation, I was sent for, and arrived about 11 o'clock P.M. The lad was visibly sinking, the attempts at inspiration being seldom and fruitless, when, without any skilled assistance, and by an unsatisfactory lamp-light, I opened the trachea and inserted the canula. After persevering with artificial respiration for some time, I had the satisfaction of getting the breathing well established, and left him in the early morning wonderfully well. As I expected, however, he died in forty-eight hours from weakness and extension of the membrane into the trachea and bronchi. That the disease was true diphtheria was proved by the fact that all the rest of the family—father, mother, and two brothers—were affected by it in the throat, some of them severely,—the father suffering for weeks afterwards from typical paralysis of arms and legs.

Case V.—A girl, aged 3 years, occurred in the practice of Dr

Steel of Comrie, who sent for me one forenoon. I found a very advanced diphtheritic condition of both tonsils, both sides of the uvula, all the fauces, and back of the pharynx. The larynx was just beginning to show symptoms of being invaded by the same disease, but not markedly so. We arranged to persevere with local and general remedies until any marked symptoms of laryngeal obstruction occurred.

In the evening I was summoned by telegram, and, aided by Dr Steel and Dr Arnot (then my assistant), I performed tracheotomy in the usual way, easily and without hæmorrhage. The breathing was soon most satisfactory, and the child relieved and of good colour, instead of being cyanosed. Within two days, however, the child sank in spite of remedies, and died partly from extension of the membrane and partly from syncope.

Case VI.—A girl, aged 6 years, had been coughing croupily for two days before I was called, when I found well-marked symptoms of true croup—the supra-sternal notch, lower ribs, and upper abdominal parietes falling in on inspiration. I prescribed the usual remedies—emetics, stimulants, expectorants, and tonics in the form of perchloride of iron and chlorate of potash, watery vapour and lime-water atomized, and compound tincture of benzoin in the respired atmosphere. Finding no improvement, but rather visible retrogression, I advised operation, having explained the object and the prospect to be entertained; and at last, after the disease had lasted five days, sanction was given. I operated in a very bad light, the only assistance afforded being by my brother-in-law, an ambulance pupil of my own. The respiration was very feeble. I had to cut through a vein, which I could not avoid, and from which hæmorrhage (excessively dark-coloured) was profuse for a time. By means of pressure this was controlled until I had got the trachea opened, and the tube inserted without any blood getting into the windpipe. I then found that there was no attempt to respire; the pulse was gone, the heart had ceased, and there was no hæmorrhage. By means of artificial respiration the patient soon recovered, and got on exceedingly well all night. Much mucus was coughed up by the tube, and occasional shreds of membrane. Directions were given that the steam should be kept up and how to clear out the tube. For some reason or other, however, the mother, with the best possible intention, left out the inner tube because the child seemed easier without it, and on the second night stopped the steam because she did not see it doing any good. This was followed by increased viscosity of the mucus and difficulty in keeping the tube clear of mucus and pieces of the membrane. Death took place three days after the operation from obstruction of the tube and the trachea below the tube.

Case VII.—A girl, aged 3 years, had suffered for nearly a week from whooping-cough and bronchitis, so perhaps I thought less of this last development than I might otherwise have done. Suddenly she developed symptoms of acute laryngeal obstruction, which, seeing that the parents were averse to operation, I tried to combat by the usual methods—emetics, expectorants, iron tonics, watery vapour with benzoin. When the child was evidently sinking, permission was given to do whatever I thought might give relief, even though it would not cure. At eight o'clock in the morning, and with the help of Dr Henry Robinson, my assistant, I performed tracheotomy with perfect success, so far as the immediate result was concerned, and a large piece of membrane was coughed out through the opening. The after-treatment could not have been better carried out. The respired air being kept well moistened by means of a bronchitis kettle, mucus was coughed up occasionally, nourishment was taken freely, and to all appearance the child was doing well. Towards afternoon, however, attacks of whooping-cough returned, and she died about midnight from syncope during a paroxysm of coughing. The tube and trachea were perfectly free.

Such, then, is a record of failure; and the question forces itself upon me,—"Am I justified, with such a record, in continuing to perform the operation as a last resource, or even as a temporary relief, to prevent death by suffocation?"

In order to answer this question it is necessary to inquire into the causes which render the operation unsuccessful. In the cases of diphtheria I did not look for anything but a fatal result at the time I operated. I do not pretend to discuss etiology, nor attempt to answer the vexed question regarding the identity, or otherwise, of croup and diphtheria. Whatever may be the opinion of pathologists, my experience leads me to make a clear distinction between them clinically. In diphtheria I think our course is plain, viz., treat the diphtheritic membrane by vigorously destroying it, keep up the patient's strength, and if symptoms of suffocation arise, open the trachea. This proceeding will, at least, allow of the more thorough application of remedies to the pharynx and upper part of the larynx. The early treatment of diphtheria is most important, and in my experience eminently successful. The early treatment of croup is also most important, but to be generally efficacious I think it leaves much to be desiderated. I have followed out carefully all the directions laid down in the best text-books, including Hilton Fagges, our latest, and by Trousseau, perhaps our earliest good authority. I have never seen a case recover when it had reached the stage at which I performed tracheotomy; and I may add that, including the tracheotomy cases, the fatality from croup is not greater, so far as I can make out, in my practice than in the practice of others. I have had very many successful cases of croup, or what I considered croup. At any rate, many cases have recovered which showed all the initial symptoms of croup.

But since I have operated, and seen expectorated the immense piece of false membrane, I am forced to doubt whether any such membrane could have existed and disappeared by medicinal treatment or natural processes.

Presuming, then, that the early treatment was carried out in accordance with the approved methods, I ask—(1.) Does the fault lie in the unskilful performance of the operation? I readily grant that my experience in operating is not large, and very much less than that of most city general practitioners. It is, perhaps, less than it should be, for I find a great tendency now-a-days among patients to go up to Edinburgh or Glasgow, or even London, for the performance of even minor operations. Whether this is due to the diffident modesty of country practitioners or the superior excellence of city surgeons I have not yet been able to form an opinion. Still, the operations which I have been allowed to perform have been in themselves uniformly successful, and these include several major amputations, several excisions of the breast and larger joints, removal of loose cartilage from the knee-joint, extraction of the lens for cataract, and operations for hare-lip. In the case of my tracheotomies I performed all the operations with care, deliberation, and comparative ease, and in only one case was there any considerable hæmorrhage. I am very far from saying that tracheotomy is an easy operation in children, or one to be performed without great care. When mentioning, in reference to the late Emperor of Germany, that tracheotomy in the adult is an easy operation, the *Lancet* laid great stress on the fact that in children it is very different. I admit this; in fact, I was sometimes dissecting quite in a hole before I reached, caught up, and opened the trachea. In every case after I had done so, however, and inserted the tube, the breathing soon became natural and, in all but the first, continued so for hours. When I remember Trousseau's statement, that tracheotomy badly performed but followed by good after-treatment is more likely to be successful than if the opposite condition obtain, I think I may without much egotism presume that the operation itself was not the cause of failure, and turn to the other factors.

(2.) Was the operation too long delayed? I believe in some of my cases it was. Had it been performed earlier I am of opinion that the patient would have had a better chance of recovery. In this connexion I cannot claim that the value of the operation can be properly judged of from my cases. Undoubtedly the majority of them were within a very short time of the fatal issue had I not operated. The delay was due, not to my own desire, but to the reluctance of the parents and relatives to give their consent, and I felt that I was not justified in very strongly urging them to consent, seeing that I could not from my own experience hold out a very hopeful prospect.

(3.) Was the after-treatment imperfect? I regret to say that in many cases it was not what I could have desired, and even my own directions may not have been entirely in accordance with recent knowledge. Most of the cases occurred in families who could not afford to pay for a skilled nurse, and would undoubtedly have been removed to a hospital had one been within reach. Unless one has had experience of provincial or rather country practice it is difficult to understand how very badly off we are for nurses. The wonder to me is that any tracheotomy cases recover if left to the care of unskilled and very often ignorant and prejudiced attendants. There is a movement on foot to provide a trained nurse for the district in which I reside, and I sincerely hope it may succeed. I feel certain that had some of my cases been in a hospital or under the care of a trained nurse the result might have been different. My endeavour was to keep the respired air saturated with moisture, to keep the tube clear, to keep up the patient's strength, and to promote secretion from the bronchial tract. I cannot say that in this my directions were carried out always satisfactorily, and I cannot altogether blame those to whom the duties were entrusted. Through ignorance and the feeling that all treatment was useless, they left off doing as they were directed just when it was most necessary.

I would here remark how very strongly I feel that something more might be done than has hitherto been recommended in the way of locally treating the false membrane in the larynx. After operation I have used various medicaments—lime water, eucalyptus, compound tincture of benzoin, carbolic acid, salicylic acid, and others—with the view of destroying or dissolving the membrane, but have not found any of them of service.

(4.) Were the cases unsuitable for operation? If so, they were, I fancy, unsuitable on account of the complications. As regards the cause of death, two died from diphtheria, one from congestion of the lungs, contracted after the operation, and bronchitis, one from intussusception suddenly, one from syncope from delay, one from syncope from hooping-cough, and only one from obstruction of the tube.

If, however, it is not justifiable to operate in any except uncomplicated cases, I fear the suitable cases will be very few in number. It does not appear to me that tracheotomy should hold a very prominent place in the treatment of laryngeal obstruction from disease if it is only to be performed in uncomplicated cases. No doubt those of us who practise in the country are handicapped by not having all the appliances for treatment and nursing that we might desire. In spite of that, however, from my experience I am led to conclude that so long as the early treatment of croup continues in such an unsatisfactory condition, the operation of tracheotomy ought to have a place in our routine practice, if not as curative, at least as capable of relieving a very distressing condition, viz., immediate death from suffocation.

I have said nothing in regard to tubage of the larynx, having had no experience of such treatment; but from what I have read in the medical periodicals, I am led to hope that the practice may yet give satisfactory results. It has this advantage, at least, that in the eyes of the relatives it is apparently a much less formidable proceeding than tracheotomy.

Mr Cathcart said Dr Thom had not mentioned whether in these cases he had made post-mortem examinations, and investigated the condition of the lungs, bronchi, and trachea. Taking a well-marked case of croup they found a false membrane, which they would probably associate with the presence of a micro-organism, but when they went back to earlier stages—what was called merely a croupy state—the symptoms were not due to false membrane, but to spasm of the glottis. He should be inclined to consider that the spasm was just one of those reflex irritations where the effect of the irritant was more marked than the irritant itself. When the irritant increased, the irritation became more marked and the membrane was thrown out. It was a very open question in his mind how far one could distinguish between croup and diphtheria in the intermediate cases, though marked cases of either were quite distinguishable. This was true of every organized group, whether of diseases, of animals, or of plants. The question of the time when operation should be performed was an important one. If early operation in all cases presenting serious symptoms were advocated, cases would probably be included which would get well without operation. There was an element, too, in the operation itself which one did not often see alluded to. When the trachea was opened the power of the patient to eject foreign matters was much impaired. If they did not have the power of closing the glottis, and so increasing the tension within the chest, they would not have the power they did have of ejecting mucus. When tracheotomy was performed, the power of increasing this tension was lost. The question suggested itself whether an artificial mechanism could be introduced into the tube which would enable the patient to cough and get rid of the mucus and other matters in the trachea. He considered that they ought to be more energetic in the free use of antiseptics locally in both diphtheria and croup. If the theory of micro-organism were true then the disease was to be considered a local one at first, and antiseptics successfully applied might have the effect of preventing it becoming constitutional.

Surgeon-Major Black asked if tubes were necessary for the purposes of the instrument. He thought it might be sufficient to keep the opening into the trachea patent by the insertion of a ring of metal which should not project into the lumen. This would get rid of the difficulty they had in connexion with the structure of the tubes.

Dr P. M'Bride was surprised to hear both Dr Thom and Mr Cathcart speak of croup and diphtheria as if they were two distinct entities. He thought it was now generally admitted that croup was diphtheria of the larynx, and that diphtheria in the box of the larynx differed from diphtheria elsewhere because of the few absorbents of the larynx. Mr Cathcart laid considerable stress on spasm of the glottis as a factor in dyspnœa, but he would like to ask if spasm of the larynx could kill. Would not syncope first occur, leading to a relaxation of the glottis and a return of the respiration. An explanation of the pseudo croup in children, which came on suddenly, usually at night, with a little hoarseness and difficulty of breathing, was found in the observations of Moldenhauer and others, who, by laryngoscopic examination, saw a swelling beneath the vocal cords, which rapidly disappeared.

Dr Hamilton Wylie thought that Surgeon-Major Black had never had a case of cellular tissue emphysema after tracheotomy, else he would not have suggested that tracheotomy tubes were of no value. He had found great benefit from the constant use of Friar's balsam in cases of diphtheria both before and after operation.

Dr James Ritchie had hoped that some member with a larger experience of operative interference would have addressed the Society. Although he had treated a large number of cases of croup and of diphtheria, he had found it necessary to operate in only eight cases—five of diphtheria and three of croup. He had also had another case of croup in which he had asked Mr Caird to operate. He did not share with Dr M'Bride the views of the London schools and of German pathologists. He believed that these are distinct diseases—that croup is a laryngitis with a simple inflammatory exudation, and that diphtheria is a specific disease—that the difference is not only a clinical one, but has a pathological basis. They differ clinically in so far that, although there may not be in diphtheria so much inflammatory action as in croup, there is much more secondary constitutional disturbance. Dr Ritchie had never seen a case of croup sink during the second week from failure of the vital powers. Diphtheritic croup is infectious; simple membranous croup is never infectious. Diphtheria is frequently followed by paralysis, croup never. Pathologically, croup bears the same relation to diphtheria of the larynx that membranous sore throat does to diphtheria of the fauces; the false membrane is more superficial, and never shows the colonies of micrococci which are found in diphtheria. Cases of croup seemed to divide themselves into two classes. First, those with a very acute onset, the child going to bed apparently well, awaking a few hours later, feverish with croupy breathing and cough. In these the croupy symptoms are due partly to inflammatory œdema partly to spasm; if treated with care, these usually recover without operation. The second class includes the cases in which a degree of cough and hoarseness have existed for some days before the more acute attack; these have had no care in the early stage, and more frequently require operation. But this is not a hard and fast division; the only fatal case of croup in the speaker's practice was one of acute onset with very high fever; death was due to extension of the disease downwards; at the post-mortem examination even the small bronchial tubes were found to be lined with false membrane. Careful examination, after hardening and staining, failed to reveal any trace of colonies of micrococci such as are found in diphtheria. Dr Ritchie's experience of tracheotomy for diphtheria was very unsatisfactory. All his five cases proved fatal—four of them by extension downwards, one of them by failure of the vital powers during the second week. Of the four cases of croup, three recovered (including the one operated on by Dr Caird); the cause of death in the fourth case has been already noted. Dr Thom had asked the question, Why are these operation cases not successful? Dr Ritchie thought that the report of Dr Thom's cases bore intrinsic evidence that the fatal result was in no case due to want of skill and care during the operation. Dr Thom was not to blame for the lateness of the operation, and he had evidently a wholesome dread of the entrance of blood into the air passages. The speaker believed that the first cause of want of success is delay in operation; so soon as the aeration of the blood is insufficient, tracheotomy should be performed. The next cause is admission of blood to the air passages, causing lobular pneumonia. The next is extension of the disease downwards; and in diphtheria failure of the vital powers is a not uncommon cause, which, however, is not confined to operation cases. Dr Ritchie agreed with Mr Cathcart that the derangement of the mechanism of coughing after tracheotomy added to the trouble of the patient. A similar alteration was observed in patients who had paralysis of one vocal cord, as well as some cause of cough. But he had always found ability to expel the secretion after tracheotomy.

Dr Thom, in reply, said he had not made any full post-mortem examination, but did make an inspection of the larynx and trachea in all of them. Tubes, he thought, were necessary for the purpose of directing the mucus outwards. He still held to the view which used to be taught in Edinburgh, that croup and diphtheria were different.

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